A Clinical Lecture

ON A CASE OF

DOUBLE PNEUMONIA FOLLOWING ON PERICARDITIS.

Delivered at the Westminster Hospital,

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GENTLEMEN,—I have much satisfaction in calling your attention to the now completed history of a case which has greatly interested us during the last week or two—that of a man named Thomas G——, who lay in Burdett ward. It is not often that we catch the very first symptoms of a pneumonia, and trace it without break during every day and almost every hour of its existence till its complete decline. And it is still more rarely that we obtain all the particulars which we have been fortunately enabled to record with accuracy in this instance. We are in a position to place side by side with the chronicle of the other symptoms from day to day a precise account of the fluctuations of (1) urea, (2) urinary water, (3) temperature, (4) pulse, (5) respiration, (6) amount of absolute alcohol administered—under the shape of brandy, (7) amount of ingoing nitrogen in food.

Thomas G——, aged thirty-one, was admitted to Burettd ward on Feb. 27th, 1874, suffering from pain in the chest, short breath, and high fever. He is a man of seemingly healthy appearance; that is to say, he does not give one the idea of a constitution impaired by any chronic disease. He told us that his father and mother were both dead, he did not know of what disease; he had ten brothers and sisters, of whom four are dead, he believes all of some kind of lung disease. He was always healthy up to seven or eight years ago, when he got herpes zoster, which spread three parts round the trunk; it must have been a severe attack, for he was ill a whole month. He may have had syphilis, for he certainly once had a sore on the penis, and he has been a good deal liable to sore-throat; but there is no satisfactory proof of it. When a child he seems to have had distinct rheumatic attacks, though never acute enough to keep him in bed. At present, though he complains of aching pains in the limbs, there is nothing that amounts to articular rheumatism.

About twelve days previous to his admission he got wet; the next day he had rigors and an evidently severe cold. By degrees he became more and more attacked with a catching pain in the cardiac region. Either on the second or the third day before admission he seems to have had a fresh rigor; at any rate, he had decided increase of pain and dyspnœa. It was these circumstances that led me to state as a fact that on Feb. 28th, the day when he began to suffer from the pneumonia of which I shall more particularly speak, he was in the fourth day of pericarditis; whereas the fuller history shows that its duration may have been much longer, possibly thirteen full days.

I have no exact report of the pulse or respiration on the day of admission; but the temperature was over 104°. Both pulse and breathing were rapid, however; but there was no striking disproportion between their rapidities. On examining the chest we at once detected that the area of cardiac dulness was increased, both vertically and transversely; a friction-sound could be heard, over the base only; lower down the sounds were muffled. Patient complained of great tenderness on pressure over the cardiac region, and pain down the whole left side. At this time there were no abnormal lung-sounds, and only some slight dulness, which might well be merely due to hypostatic congestion, at the bases behind.

On the afternoon of the next day (Feb. 28th) it was obvious that something fresh had occurred. The patient's face was brightly flushed, and he had an extremely troublesome cough; but what immediately struck me was the immense increase in the frequency of the respiration, without any corresponding increase in the frequency of the pulse. The breathing was now 66, while the pulse was only 122 (and I may as well say that this peculiar high ratio of respiration to pulse was unusually manifest throughout the case; for on the second day the respiration reached 71 per minute, and even as late as the eighth day it was as high as 57, although the pulse had fallen to 84). Now such a ratio of breathing to pulse is highly suggestive of pneumonia; and on making an examination, on the first day (Feb. 28th) of its appearance, we detected fine crepitation over an area of about 2½ in. at the base of each lung, posteriorly. On testing the urine with acid and with solution of nitrate of silver we got merely the faintest cloud, instead of a dense precipitate; showing the almost entire absence of chlorides—an important confirmatory sign of the pneumonic state, when taken with the other facts, though, as you are aware, it occurs in several other pyrexial diseases. On the following day the right lung was unchanged as regards the extent of dulness on percussion; but we found the crepitation replaced by bronchial breathing. The corresponding portion of the left lung was also the seat of bronchial breathing; but the dulness had extended a good deal higher up, and in this new area of inflammation fine crepitation was heard. The sputa had now taken on the characteristic appearance: they were somewhat copious, excessively tenacious, and mixed with blood in that intimate manner which has been so often pointed out to you in cases of pneumonia. On the next day (third of pneumonia) the pericarditis had passed into a new and interesting stage: the area of cardiac dulness was diminishing, and friction-sounds were heard over a considerable portion of the heart. This was *returning* friction, indicating the absorption of effused fluid, which again permitted the pericardial surfaces to rub together. At the same time we detected a patch of lung-consolidation outside and below the left nipple, which had not been noticed before, and was evidently later in date than the basal pneumonia. On the following day (fourth of pneumonia) there was bronchial breathing over this patch (as large as a crown-piece) outside and below left nipple. By this time the dulness of both bases, posteriorly, had a good deal diminished. In the right lung the inflammatory process had never extended beyond the moderate area over which we first detected it; and at present, the sound of "returning crepitation" over both bases showed that resolution had commenced. On the following day (fifth of pneumonia) my clinical clerk, Mr. De Brent, reports as follows:—"Patient propped up in bed. No bulging of cardiac region; no increase of area of cardiac dulness vertically; no precordial friction-sounds. Heart-sounds normal at base and apex; but the impulse is not seen and not distinctly felt on applying the hand. Fair resonance of both (lung) bases; fine crepitation at left base. Fine crepitation and pleuritic friction over lower half left front. Expectoration excessive, green and stringy; cough violent. Temperature (10 A.M.) 100·8°; pulse 90; respiration 57, shallow." On the following day (sixth of pneumonia) the cough was easier; pulse, temperature, sputa, and respiration about the same. The thirst was less, and he was beginning to perspire freely.

On March 7th (eighth day of pneumonia) there were no longer any friction or crepitation sounds to be heard, but there was persistence of a small patch of bronchial breathing outside and below left nipple. He was reported as sleeping fairly well. Chlorides returning to the urine. Pulse 84; respiration 57; mean temperature of twenty-four hours 100·2°.

Two days later the lung and heart sounds were free of any abnormalities whatever, except that the latter were somewhat muffled, probably from progressive consolidation of pericardial adhesions.

So far as the history of the physical signs is concerned, the case ends here, as there was nothing abnormal to be reported afterwards. But the cough and expectoration did not altogether cease, and it is noteworthy that not till the twelfth day had the respiration fallen as low as 28, which is still, I need not tell you, considerably above the natural standard.

You have heard what was the physical progress, so to speak, of the disease; let me now say a few words on its broad vital characteristics before proceeding to discuss those details which are set out in the chart which I lay before you.

First, then, we must notice the fact that this pneumonia

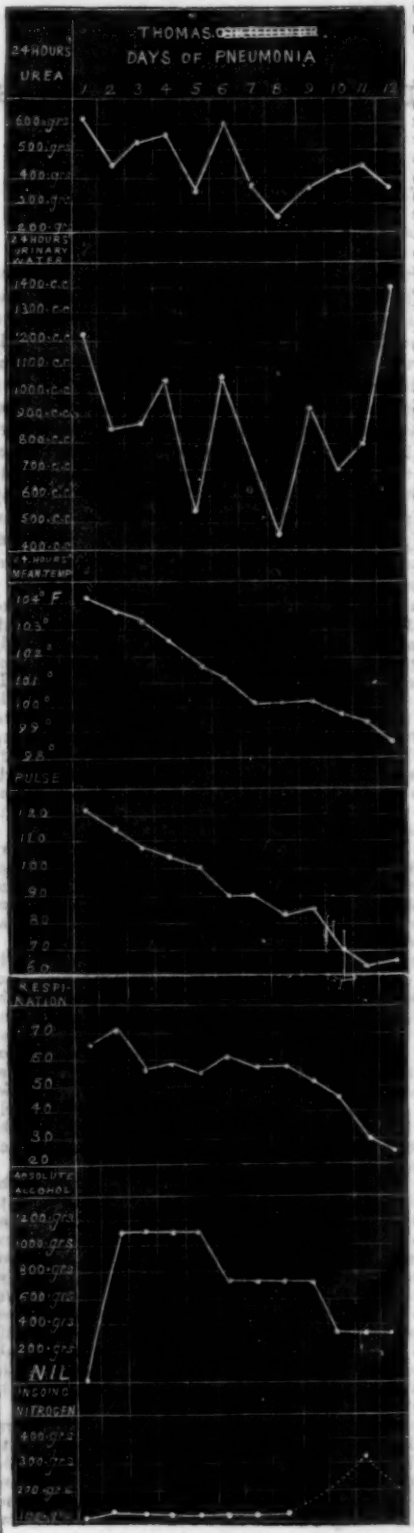
began under bad auspices; for (*a*) the man was already suffering from pericarditis, (*b*) he might well be suspected of having a rheumatic diathesis, and (*c*) the great dyspnœa from which he suffered seemed to indicate a very limited power of tolerance on the part of his system. Secondly, notwithstanding all this, the disease really ran a perfectly simple course, and (as you will see more vividly when you inspect the curves of our diagram) the fall of the pulse and the *mean daily* temperature was continuous and unbroken. And it is particularly to be noticed that the progress of the lung tissue through the various stages of inflammation, consolidation, and resolution was remarkably rapid; so that, although the disease was double and showed a tendency to spread in the left lung, and a large portion of that lung was at one time or another engaged, the whole of the morbid physical signs had disappeared by the eighth or ninth day. Moreover, the pleura, which one would quite have expected to suffer liquid effusion, only poured forth adhesive lymph; and this pleurisy in no way gave us any trouble beyond a good deal of catching pain which it occasioned, and which obliged us to use opium pretty freely.

Looking at the unusually even progress of the descent of pulse and temperature, it is difficult to fix upon any day or days as "critical." But there was at least one day—March 5th (sixth day of pneumonia)—on which the patient was found to be perspiring very freely, and from this point he certainly slept better than he had done before. It is worth while also to notice that on this day there was a sudden increase in the urea-elimination; but I shall show you that this was probably not truly critical, or only so in part.

We have now to study those details which are afforded by our charts of the urea-discharge, urinary water, pulse, temperature, breathing, alcohol, and food-supply: these are full of interest.

I have already spoken to you of the remarkably continuous fall of pulse and temperature throughout the progress of the malady. You have it now very clearly under your eyes. Of course you will understand that there really were other small *diurnal* fluctuations of the temperature, and that the readings before you represent the mean of three daily observations taken at as even intervals from each other as possible. When looked at thus, the heat of the body is found to diminish with scarcely a pause or check throughout. That the temperature should fall *continuously from the first day* (on which it once attained to 104·9°) is uncommon in pneumonia; still more so when the inflammation goes on extending its area during a day or two, as it did in this case. This fact becomes of double significance when we consider what kind of subject the patient was, how he would have been treated some twenty-five years ago, and how he actually was treated. He is a labourer in the strength of settled manhood, and has never been shaken by any serious disease. The acuteness of the symptoms, and especially the degree of dyspnœa, in such a subject, would have seemed to our predecessors an infallible argument for what they considered the only effective antiphlogistic treatment—viz., free venesection, and the use either of mercury to salivation or of large doses of antimony, or both of these. We did nothing of the kind. We gave opium (half a grain every four hours) to keep the pleuritic pain in some check; and we did attempt to relieve the dry heat of skin* by moderate doses of acetate of ammonia; but in fact the skin hardly perspired at all till the sixth day; and, instead of bleeding, we gave the man six ounces of brandy daily till the temperature had fallen to a daily mean of 101·8°; then we cut it down to four ounces; three days later we reduced it to two ounces. After the twelfth day all stimulant was taken off; and at present (March 13th) he is taking the ordinary full diet of the hospital without any adjunct whatever. That was how we treated "sthenic" pneumonia, rendered particularly hazardous by reason of the pre-existing pericarditis. You can judge for yourselves whether the old depletive plan would have been likely to succeed better.

But, in fact, gentlemen, alcohol in proper doses and on proper occasions is the truest "antiphlogistic"—the most direct opponent of the pyrexial process and its objectionable results—which we possess. Its influence upon the pulse and temperature in the present case was, I think,



unmistakable. But there is another test, which also demonstrates the beneficial influence of the alcohol in this case—I mean the course of the urea-elimination. It is not necessary for me to repeat to you all that you have so frequently heard about the urea-discharge in pyrexial diseases, and the dependence of that discharge upon disintegration of the nitrogenised tissues. You will remember that, while admitting to the full that urea, once generated by such tissue disintegration, had better be promptly eliminated, I have maintained that if we can suppress or reduce the *formation* of urea we ought by all means to do so; and that we can do so, very often, with the best effect, by means of alcohol. I do not think there can be a doubt that we did so in this case. Before we began the brandy the twenty-four hours'

* If I had been very earnestly bent upon this it could soon have been done with aconite, compared with which all other "diaphoretics" are feeble and disappointing.

urea amounted to 609·024 grains; the first day of six ounces of brandy it fell to 447·876 grains; and though it crept up again in the next two days, it did not get beyond the very moderate level (for acute pneumonia) of 556·3 grains, and then fell to 380·72. We now cut down the brandy to four ounces, and the next twenty-four hours saw a momentary leap of the urea-discharge to 600·912 grains. There is some reason to think that a part of this increase may have been "critical"; but, considering the calm and equable course of pulse and temperature, it is impossible to suppose that a crisis existed sufficient to account for the whole of the increase. However, it was but for one day. On the following day only 393·432 grains of urea were passed, and on the next to that only 266·52 grains. Up to this point he had been fed as follows:—On the first day he had beef-tea and milk, which contained rather more than 100 grains of nitrogen; for the next seven days he was kept strictly on a diet of eighty ounces of milk, containing about 120 grains of nitrogen—a quantity which, reckoned in urea, would correspond to 240 grains. From the beginning of the ninth day, however, it was impossible to restrain his hunger, and he was perforce allowed, besides his milk, a quantity of bread which increased every day.* Under the influence of this it is interesting to observe that the urea steadily rose to 449 grains. He was now taking a diet very rich in nitrogen. Being then, to save trouble, put on the "middle" diet of the hospital, which contains a good deal less nitrogen, the urea fell next day to 371·28 grains. So, in these last four days of the urea record, it is evident that you see no influence of disease, but simply that of varying nitrogen-supply in food.

A subsidiary but still an interesting matter was the fluctuations of the urinary water. It will be seen that these showed a certain very rough general agreement with the fluctuations of urea; but the agreement was not at all close, and on some days there was the reverse of agreement, as for instance on the twelfth day, when the water increased from 800 to 1400 cubic centimetres, while the urea considerably diminished. It is interesting to observe that, during the whole of the most febrile period, when the patient was taking some 2240 cubic centimetres of milk, the urinary water only averaged 740 c. c. Until the last two days of the pyrexial period very little was added to this by skin excretion, sensible or insensible. The deficiency must have been made up by the water exhaled in vapour from the lungs. You will remember that the frequency of the respiration was very great, and in this way a large quantity of water must necessarily have been got rid of in the form of vapour.

To return to the subject of the urea-elimination. This was kept down at a level which was exceedingly moderate for acute pneumonia, the disease which of all others, with the possible exception of typhus, is distinguished by the large urea-discharge by which it is attended. From 700 to 900 grains of urea daily is probably not too high an estimate for acute pneumonia running its natural course in an adult man, and very much larger quantities have often been observed; thus Parkes mentions one case in which the urea was 1321 grains on the sixth day, 1349 grains on the eighth, and the same amount on the ninth day. I myself lately observed a case in St. Thomas's Hospital (kindly put at my disposal by Dr. Clapton), in which pneumonia, coming on after several days of pleurisy, was allowed to run its course without special medication up to the sixth day, by which time the urea had mounted by daily increments to 1348 grains. Alcohol (3 oz. absolute daily) was then administered, and although the temperature continued almost undiminished for several days, the urea fell on the first day of stimulation to 1317, and on the succeeding days to 552·1, 467·2, 189·15 grains, and then slowly rose again to 468·4 grains on the fifteenth day, at which time the patient was fully convalescent, and eating a mixed diet. During the pyrexial period he had been kept strictly on milk and beef-tea, two pints each; the exact figures in nitrogen I cannot give you here, but they will be ultimately published.

Is the diminution of urea-discharge produced in such cases a mere temporary *retention* of urea which has been already formed within the body? Because, if so, we might expect injury rather than benefit; and, indeed, various authors of high standing have declared that this is the case, and that a large urea-discharge is to be favoured rather than suppressed. But you have seen very plainly that the phenomenon was *not* one of mere retention in the case related to-day; and, on the other hand, you have recently seen two cases of typhoid fever in our wards* in which (without any interference from stimulants) a high pyrexia, accompanied with a low urea-discharge, ran a very favourable course. The truth seems to be that, although urea once formed had best be eliminated, there is no reason at all for congratulating ourselves upon the extensive destruction of nitrogenised tissues which such large quantities of urea denote; on the contrary, there is every ground for seeking all legitimate means for reducing this serious and often dangerous expenditure of tissues (like that of the heart and the respiratory muscles), the integrity of which is important to the most essential vital functions. Before long I shall publish a large quantity of evidence by which this opinion is supported; at present it is sufficient for me to call your attention to my rule of practice, which you will see exemplified in the wards on many future occasions. That rule is, that in any acute pyrexial disease in which *high temperature is conjoined with a large urea-discharge*, that conjunction is the strongest *primâ facie* reason for the administration of alcohol, and that a reduction of heat and urea-discharge (or of urea-discharge alone, provided no toxic symptoms follow) is to be regarded as evidence that the stimulation has been rightly adopted.

Let me add, lest you should be frightened at the idea of daily analyses of urine in busy general practice, that a very good rough notion of the amount of urea may be obtained by merely measuring each twenty-four hours' collected urine, and taking the specific gravity of a sample of the whole when mixed. You will soon find out this by practice in the wards. In the above case the urea was calculated daily, by Liebig's process.

* In the food chart the *dotted* part of the nitrogen line indicates this increase of nitrogen supply in a merely conjectural manner.

ABSTRACT
OF THE
Gulstonian Lectures
ON THE
ORIGIN AND RELATIONS OF NEW GROWTHS.

Delivered at the Royal College of Physicians,

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LECTURE III.

THE word "cancer" has long been used in two distinct senses:—(1) Tumours or growths having particular morbid properties, or involving a particular kind of danger to life; (2) tumours or growths having a particular kind of structure. Of late years a third meaning has been introduced—a tumour or growth originating in a particular part or tissue. The first real attempt at an actual anatomical classification of tumours is due to Abernethy. He speaks of cancer as a carcinomatous sarcoma, distinguishing it from medullary sarcoma, which he says has neither the hardness nor the disposition to ulcerate which characterise cancer. Cancer, again, he says, taking the type of it from the female breast, either appears to be part of the new formation or else the mammary gland itself is the nidus of the disease. In either case it begins in a small spot, and extends further in all directions. Its progress, even when slow, is unchecked; it excites contiguous parts to the same diseased action. The other characteristics are inequality of surface, ulceration, with destruction of the substance by a process partly sloughing and partly ulcerative. An attempt at reparation is made; a fungus of peculiar hardness is formed; still the diseased action never stops, nor does the part ever become healthy. Further, the diseased action extends by means of the absorbing vessels, which become affected by some matter

* Vide *Practitioner* for March, "On Tissue-destruction in the Febrile State," &c.

absorbed from the seat of the disease; and the same appearances are sometimes seen even in distant parts. As to structure, the diseased part is generally hard and intermixed with fibrous bands, which run out into the surrounding tissue. Medullary sarcoma, he says, is chiefly distinguished by not affecting contiguous parts, and not ulcerating or breaking down in a mass. These characters nearly accord with what we now regard as the characters of malignant tumours; and succeeding observers have not added much to the description. All the characters named by Paget as those of malignant tumours have been anticipated by Abernethy, except the unlikeness of cancer-tissue to any element of the normal body; and that is a character of an entirely different class, hardly logically to be put in the same list. For a long time the notion of cancer was derived from the progress and life history of the disease, quite unconnected with structure. One could almost wish it possible for it to remain so. Making allowance for the progress of anatomical knowledge, and correcting some features in the old description which were drawn too exclusively from surgical experience, it will still be possible to frame a physiological or clinical definition as accurate as can be expected in matters so variable as growth. Most of the characters insisted on by Abernethy are absolutely decisive. Hardness and ulceration are less important and less general in their application. Soft tumours possess all the other characters ascribed to cancer. Ulceration only occurs in tumours near the surface; the analogous process in deeply-seated tumours is softening, and in our definition we may substitute for malignancy softening or rapid decay. The characteristics above mentioned are not the tests of malignancy; rather they *are* malignancy. But in this definition there is not a word of anatomy. One difficulty in our way is that the characteristics are not manifest at all stages of the morbid growth and history. A tumour may manifest none of the characters in question, and yet we may know from collateral evidence that it would go on to develop them if not interrupted by surgical interference or death. What name should be given to such a tumour? It is not at the time malignant. In cases of cancer of the pylorus, for instance, we see that many of them do not really exhibit the malignant properties described by Abernethy and Paget. But we know that, if they did not by their situation cause inanition and early death, they would in all probability develop those characters. No one type of structure is peculiar to or characteristic of malignant growth. We cannot draw a direct inference from structure to malignant property any more than we can infer the medicinal effects of a drug from its chemical composition. Nor can we from the fact that a tumour has manifested malignant properties draw a positive conclusion as to structure any more than from the fact that an animal is predacious or carnivorous we can refer it to any one order, since predacious or carnivorous species occur in several orders. The clinical or physiological definition of cancer or the classification of tumours on this basis cannot coincide with a definition or classification derived from structure. Nor can they be strictly contradictory. They are not in the same plane. If the practitioner asks whether a tumour is malignant, the histologist can only reply that it is of such and such a structure. The best way seems to be to classify and name objects according to their structural character, and so to gain a key to the recorded experience respecting their properties. If we begin with physiological characters we are on less solid ground, for these characters may not be developed at the moment, and may never become so.

In what sense are we to use the term cancer? Some would use it as the substantive of the adjective malignant; but this has no advantage over the term malignant tumour. Others use the Greek form carcinoma for the anatomical species, leaving cancer to be a clinical term; and this is at present a possible compromise, since we shall not for a long time prevent the use of the word in two senses. It is impossible, however, to avoid attempting an anatomical definition. According to Virchow, a cancer is essentially a tumour composed of an alveolar fibrous framework or stroma, containing cells of an epitheloid type arranged in a disorderly manner. These cells are closely pressed together, with no intercellular substance. The fibrous framework contains bloodvessels and connective-tissue corpuscles, and sometimes a large number of lymphoid or migratory corpuscles are seen constituting a sort of intercellular infiltration. The essential feature of cancer thus understood I believe to be neither the one of these elements nor the other, but the union of the two—i.e., of a fibrous framework, obviously a connective-tissue structure, and elements which differ, at least in form, from any connective tissue. Some, attaching undue importance to one of these elements, have described cancer as fibroma, with cell-containing alveoli; others (like Waldeyer), as a growth essentially epithelial, but taking place in the midst of connective-tissue structure. They regard the two parts as distinct in their origin, and proceeding from different layers in the embryo.

The arguments brought by Waldeyer and the modern school in support of their theory of the origin of cancer are somewhat as follows:—First, the character of the cancer-cells, which they say resemble those of the organ near which they arise [several illustrations of which were mentioned]. These analogies are certainly true in the case of primary cancer in the localities mentioned, and comparing these cases with the growth of superficial epithelium there is certainly much probability in any theory which assimilates their origin. And if, as Virchow supposes, all these classes of structure arise from the connective tissue, we must assume very peculiar circumstances in each case to explain the strange similarity of the cancer to the adjacent structure. The second argument is derived from direct observation of the relation of tissues at the growing margin of tumours. On making a section through the margin of an epithelioma we find appearances which seem to show that the process essentially consists in a growing inwards of masses of epithelium into the deep parts of the skin and into the connective tissue, in the growing margin of cancer this tissue being in all cases not normal but modified by proliferation so as to form a nucleated structure, or one infiltrated with small cells. In the growing margin of cancer we also meet with two kinds of products—a proliferation of small cells and masses of epitheloid cells resembling those of mature cancer. According to Virchow there are transitional forms between these, but according to the other school there are two kinds of development which go on quite independently. I have never been able to see any such transitional forms. The third point urged in favour of the exclusively epithelial origin of cancer is the alleged fact that this growth never occurs except in organs or parts which contain normally epithelium. This assertion seems startling, for it denies the possibility of a cancer in any of the extremities unless growing from the skin, of any bone cancer, of primary cancer of lymphatic glands, or primary cancer of the brain, since in the lymphatics and in the membranes of the brain the elements which have the form of epithelium are not regarded, on this view, as strictly epithelium, but as so-called endothelium, having, that is to say, a different embryonic origin. Tumours may of course grow from all these parts which are malignant in the highest degree. What is denied is that tumours arise there having a quasi-glandular structure which is regarded as constituting true cancer. With regard to epithelioma, it is the rarest possible circumstance for this growth to occur elsewhere than on epithelial surfaces. Not more than three or four clearly-described cases are on record in an experience of twenty or thirty years. The modern school try to explain away these cases. In the case of lymphatic glands near the surface they suppose that the cancer may have begun in the skin and spread downwards before it was noticed. Other cases they suppose may have been dermoid cysts, or involutions of integument at an early period. These explanations seem forced. Still the small number of such cases is very remarkable when we consider that since surgeons have been in the habit of using the microscope some hundreds, if not thousands, of such tumours must have been removed. With regard to cancer, as distinguished from epithelioma, a similar assertion has been made, but cases of this kind also are no doubt very rare. At the same time I think rather more cases of cancer of this kind have been known than of epithelioma. Two years ago, in the Pathological Transactions, a case was described by Mr. Anderson, of St. Thomas's Hospital, of a sarcoma which was removed and recurred many times, and on the last occasion on which it was observed it had a structure which was regarded as cancerous. But I think the stroma (as shown in the diagram) is not quite the same as that of cancer. Another case, represented in a diagram—sarcoma upon the scapula—would also be described by Virchow and others

as cancer. I lately examined the case of a primary tumour of the leg, removed by Mr. Croft, and afterwards a similar tumour arose in the stump, and finally within the pelvis, and from all these tumours we obtained preparations showing precisely what is regarded as cancerous structure. There is no doubt that sarcoma may often assume an alveolar arrangement, and the question will then arise whether alveolar sarcoma and true cancer are at all different. The above cases are almost the only ones I have been able to find of a tumour with a distinctly alveolar structure arising from the limbs, from bones, from fibrous tissue, or any connective-tissue structure, which have been recorded of late years, or in which the structure has actually been figured.

One other large class of tumours, commonly described as cancerous, are those arising from lymphatic glands,—those, for instance, which arise in the mediastinum and the bronchial glands, formerly the intra-thoracic cancers which have been brought before us so completely by Dr. Bennett. Many of these tumours are in the highest degree malignant, and that they should be called cancer in the physiological sense cannot be doubted. Their local infectiveness is sometimes extraordinary, equal to, if not surpassing, mammary cancer. One case is mentioned in which a tumour of this kind, beginning at the mediastinum, affected a great part of the lung, also the pericardium, and formed masses covering the heart. Most of these tumours, however, have a very simple structure. The cells are precisely like those of the lymphatic glands, and what stroma or connective-tissue structure there is, has very much the simple outline of lymphatic glands. I cannot say that I have seen a case in which the structure was precisely what would be called cancer in the restricted sense I have mentioned. Still such tumours have been described by highly competent observers. I conclude, then, that it is possible, though not common, for mediastinal tumours to assume a type of alveolar growth called cancer, and that there are such things as cancers arising from connective tissue. The law of development on which Remak insisted is by no means absolutely proved by the laws of the occurrence of tumours, since growths resembling those which arise from epithelial structures may also, though rarely, be derived from the connective tissue. I conclude that distinct types of structure may be marked out, which it is convenient to call cancer; that this arises in a great majority of cases from epithelial structures, surfaces, or glands; that there are two classes of these—namely, epithelioma and gland cancer, and that these are both highly malignant, though in not quite equal degrees. This enumeration does not exhaust the class of malignant tumours. Certain other forms of sarcoma, lymphoma, or lympho-sarcoma are also extremely malignant, and there are rarer cases in which simple tumours also are malignant. The property of malignancy does not belong to any single type of growth, or even to several types, but is rather met with in several classes in unequal degrees. The most malignant form of cancer is that called encephaloid, next the round-celled or medullary sarcoma, and with which I should put lymphatic tumours. Colloid cancer is malignant in a less degree. Next come epithelioma, osteoid cancer, spindle-celled sarcoma, glioma, myeloid sarcoma, myxoma, enchondroma, fibroma, and, finally, the simple-tissue tumours which are malignant only in the very rarest instances.

A few words as to secondary tumours, which exhibit in many respects the characters of cancer. The channel of communication between primary and secondary growths is very often through the lymphatics, but sometimes directly by the bloodvessels. Cancer of the abdominal organs may in this way be carried by the portal system into the liver. With regard to other parts of the body, secondary tumours more often arise in the lungs. The evidence of vascular distribution is, in fact, precisely of the same kind as in the case of pyæmia, but even stronger, for we almost invariably find that when the poisonous matter is introduced in any way into the blood, it is arrested by the first set of capillaries through which it has to pass. What is the virus thus transmitted? The most obvious supposition is that an actual transplantation of texture—that is, of cells—takes place, and that these cells take root and vegetate in their new site. Virchow has supposed that some seminal principle, or seminum, is carried from one part to another, and stimulates the tissues of the new part to a new growth similar to that of a primary tumour; but no such substance as this is actually known, and it is difficult to form any conception of what it would be like. It must be something that can only be compared to the spermatic cells. This comparison is, indeed, made by a German pathologist, but we should be cautious in admitting such purely hypothetical statements. Waldeyer says that secondary growths arise wholly from a germination of transplanted cells. This is certainly too absolute and hasty a statement. I referred to a case in the first lecture where there were undoubtedly secondary growths produced from germination of the tissues of the omentum—a process I should like to compare with that of the skin-grafting, in which there has been observed both a germination of transplanted cells and a germination of the tissues on which they are transplanted. In what way the cells influence the growth of their new matrix is of course doubtful, but it seems more likely that there should be something resembling the conjugation of sexual elements than that there should be some unknown physiological stimulus. In any case, the primary tumour must be regarded as the centre and origin, and the others as secondary products. If this be the case, we may perhaps see some answer to the question which has often been asked, whether cancer be a local or a constitutional disease. The answer to this question might be the whole series of facts which I have already laid before you. This is the moral of the whole story, and if the tale does not tell its own moral, it must have been very badly told.

Address
TO
THE HUNTERIAN SOCIETY.*

Delivered March 12th, 1874,

BY ROBERT BARNES, M.D.,
PRESIDENT.

FELLOW-MEMBERS OF THE HUNTERIAN SOCIETY,—I am sure you will believe that no ordinary obstacle would have prevented me from offering at the proper time my grateful acknowledgments for the signal honour you have conferred upon me by electing me your President. The only apology I will offer is, that the many trying preoccupations which have recently pressed upon me prevent me from expressing my feelings to you in a becoming manner. I feel your kindness the more deeply because it proves that you have not forgotten me in my migration to the West. I can only assure you in return that "*cælum non animum mutavi.*" As time wears on, as the aspirations of our early career give way to the experience and reflection of maturity, there are few things that bring more solid satisfaction than to find that we have earned and retained the confidence and the friendship of those with whom we have worked. Many are the pleasant evenings which I have passed here—evenings rendered doubly profitable by that happy blending of friendly intercourse and scientific discussion which so eminently distinguishes the Hunterian Society. I do not know any Society in which the graceful art of mixing the useful with the sweet is better understood. Every Society has a character of its own impressed upon it by the conditions of its existence, and, I may add, by the spirit which presided over its foundation. Imagination, carried back to the day when our revered friend Dr. Cooke conceived the idea of founding this Society, easily realises what his earnest and sympathetic mind discovered amongst his brethren practising in this busy centre of life. There was yearning for friendly association; there was thirst for knowledge. He saw, or rather he felt, that these two imperious longings, struggling as they did in the same breasts, were to be satisfied by one and the same measure—namely, by bringing the men together.

De Quincey has well said, "If in this world there is one misery having no relief, it is the pressure on the heart from

* Published at the request of the Society.

the Incommunicable." This misery—for such it must have been—weighed with peculiar oppression upon the hard-worked medical practitioners of the East of London, cut off as they are to a great extent from association with their brethren in the West, where many Societies flourish. That pressure on the heart of the Eastern medical world has found relief in the working of the Hunterian Society.

Gentlemen, whenever I think of this Society I cannot help paying my homage to its founder. Founders are of the elect. The creative power, the divine attribute, descends only upon a few men of generous mould. To have known such men is to feel oneself ennobled. The tendency and character of the Society are summed up in its history. And I think it is not paying undue honour to that wise and good old man, whom we all of us remember with affection, to say that its history was foreshadowed in his mind when, more than fifty years ago, he founded the Hunterian Society. Herein it is that we may realise how useful and ennobling a thing it is to have a history whose course was determined from the beginning by the inherent necessity of representing the mind, intellectual and moral, of such a man as Dr. Cooke. The initiative faculty, which prompts to the conception of new designs, is often at its strongest in young men. But it is more rare to find united with it the power of informing other minds with the creative thought, and of persuading other men to join in giving to that thought the name of action. Yet both these qualities were happily united in Dr. Cooke. He succeeded because by his intelligence he inspired respect; because by his clearness of judgment, strength of purpose, and sterling honesty he inspired confidence; and because by his kindness of heart he charmed his associates into sympathy with his aspirations.

It is given to few men to witness such complete crowning of the edifice of their early ambition as was vouchsafed to Dr. Cooke. During half a century the Society, whose starting he had inaugurated, grew and formed itself under his judicious administration, by his scientific co-operation, and by his bright example of gentle courtesy in all his personal relations with his fellow-members. I delight to think that, cherishing his memory, we are still animated by the same love of science applied to the relief of human suffering, and that we still feel the gentle influence of a spirit whose every movement was fruitful of good.

The traditions and character of the Hunterian Society have been admirably sustained by a long line of presidents, all of whom have been or are men of mark, and many of whom may be said to have been in a large measure the scientific offspring of the Society. I do not pretend to repeat what has been so well told by Dr. Fotherby in an oration which records a history that will always be read with pleasure by present and future members of the Hunterian Society; but I hope I shall not trespass too much on your indulgence if I offer a few brief observations on the place which the Hunterian Society appears to occupy amongst the kindred Societies of this metropolis. Medical Societies, in their individual and aggregate character, constitute what may be called the Republic of Medical Science, exerting a powerful influence over the organisation and conduct of the members of the profession. They are practically open to every member of the profession. In them every man has his opportunity. They are great Schools of Medicine, in which the higher education of its members is carried on after they have left the hospitals. It is one of the greatest charms of practice in a large town—one that compensates for the loss of the glories of nature and the serenity of a country life—that we can meet together to talk over our experiences, and to enlarge our knowledge by imparting what we have learned to each other. The country practitioner, pursuing his useful toil in a comparatively isolated circle, can hardly realise as we can the truth of these grand words of Shelley:

> "True love in this differs from gold and clay,
> That to divide is not to take away.
> Love is like understanding, that grows bright
> Gazing on many truths."

No words could more happily express the general truth of the oneness of Love and Understanding; and no Society can show a better example of it in practice than ours.

It is in our Medical Societies that men are formed, trained, and moulded to their work. By meeting men who have drawn their elementary knowledge from different hospitals and different schools we learn to discard local prejudices, and to cull the good from many sources. The Medical Societies offer the best field for the cultivation of those qualities which make up an effective teacher. And more than this: having trained and fitted a man to be a teacher, having given him the opportunity of proving his fitness in the eyes of the most competent judges, their testimony will often carry him by irresistible opinion to the coveted place. In this way our Medical Societies exert a most wholesome influence in supporting merit against nepotism. No vaporous windbag can successfully encounter the ordeal of a Medical Society. No dishonest quibbler can escape reprobation and contempt.

In the West, where there is more leisure, more time to reflect, Societies abound. In the East, where the practical work is more engrossing, Societies are few. Hence, in the West, we find medical science is more cultivated on the principal of subdivision of labour. Medical science, whose elements are really indissoluble, is by an arbitrary process cut up into parts, to each of which a special Society is devoted. That great benefits result from this practice cannot be denied. But a Society in which medicine can be studied in its grand unity, in which those whose practice and studies are more or less bent in a special direction can meet on common ground, and who are thus compelled to see the mutual relations of the different branches of their art, is after all the most instructive, and offers the purest type of what a Medical Society should be. Such is the constitution of the Hunterian Society. It elicits and diffuses a knowledge of medicine, surgery, and obstetrics in their abstract, scientific, clinical, and pathological aspects. But it is above all a clinical Society. Composed of men actively engaged in the practical work of coping with disease, the want that is most urgently felt is to learn from each other more of the nature of disease, and how to improve their capacity to deal with it. Our discussions here, then, are of an eminently practical character. And, owing partly to another peculiar feature of this Society, these discussions exhibit more of the spontaneous life and frankness of friendly conversation than of the more formal and studied debates of other Societies. Our proceedings are not as a rule published in the medical journals. I am far from contending that this want of publicity should be the rule; but I am not sorry that there is one Society at least where the members can interchange thoughts freely, unrestrained by the fear of criticism beyond the circle of their friends. In such meetings as ours no one, however young, however small his experience and great his diffidence, is deterred from stating facts which seem to him pertinent to the point in discussion, or from asking questions which often elicit information welcome to all, and which help to sustain the interest of debate. And now and then our elastic constitution has enabled us to get up a debate which has attracted men of reputation from all parts of the town, and the reports of which have been read with eagerness wherever our medical journals penetrate. I believe that it is wise to adhere to the customs under which institutions have flourished. A little reflection will often show that those customs are the direct results and expression of the conditions under which the institutions exist.

Whilst then, as a general principle, I admit that the publication of a Society's proceedings is a debt due to science, and to our brethren, who by the force of circumstances are precluded from being present at its discussions, I would cherish the old customs which have sprung up in this Society, which mark its individuality, and which meet our wants. An institution which has stood the wear and tear of more than half a century may be presumed to possess a sound constitution. No profession studies constitutions more carefully than we do. Our patients trust us because, rightly or wrongly, they acquire the belief that by observation we have learned to "know their constitutions." At any rate we know the constitution of the Hunterian Society. Its conservation is in our hands. There is no reason to doubt that, if we refrain from rashly trying it, that constitution will survive for many half-centuries, diffusing the blessings of friendship and of knowledge to many generations of our successors.

A WIDOW named Lenoir, who died in Paris last week, bequeathed ten millions of francs to found an asylum for old and infirm people.

HOSPITALISM:

AND STATISTICS OF THE LATE OUTBREAK IN LINCOLN COUNTY HOSPITAL.

BY CHARLES BROOK, M.R.C.S. &c.,
SURGEON TO THE HOSPITAL.

AFTER the refutation of the term "hospitalism" by such eminent men as have discussed the matter at the Clinical Society, my only excuse for using it lies in the fact that, whilst they have asserted the presence of pyæmia to be as general in private as in hospital practice, they have not attempted to prove that there does not at times exist, in some defective hospitals, a *materies morbi* which affects wounds or broken surfaces, and produces one or all of three infectious disorders—phagedæna, erysipelas, and pyæmia.

We know that some hospitals seldom or never suffer from these attacks, whilst others suffer very frequently; and surely there must be something connected with the hospitals themselves—faults either of construction, drainage, or management of wounds by either surgeons or nurses—to produce this result. I think therefore that Mr. Erichsen was justified in using the term "hospitalism" as applied to such cases engendered in defective buildings; but the causes of these outbreaks are, I believe, entirely avoidable, and therefore hospitalism should not exist.

Mr. Callender very justly disagrees with the general use of the term pyæmia, and certainly the pyæmia which we see in our hospital is very different in character from the so-called pyæmia in private practice. I can call to mind several cases of the latter in my own practice, and have had reason to welcome its advent: as in the case of a young married lady, who, on the second day after confinement, was seized with acute mania; this was checked by a large dose of opium (four grains) in the rectum; pelvic cellulitis and tympanites ensued, with intense sensibility of the nervous system; for several days her life hung in the balance, but pain came in the upper arm, and she improved immediately, an abscess forming after a week or so. I think the local inflammation and deposit of pus saved my patient's life. I have at the present time the case of a young lady with typhoid fever; considerable lung complication in the early stage; the typhoid stage well marked for three weeks. Improvement of general symptoms followed, but accompanied with frequent rigors and sweatings for a week, when she improved rapidly, and complained of acute pain in one knee. After a few days it became swollen; and now, with a cheerful countenance, moist tongue, and quiet pulse, the knee is going down, and her ankle is swelling. Here, again, the pyæmia came and relieved her just when I was despairing of her life. In these cases there was no septic poisoning, but a distinct febrile disorder, which was relieved by a deposit of pus; and if they are to be called pyæmia, they differ vastly from the last case of pyæmia occurring in my hospital practice, in which an old man with a broken leg, on which was a small chronic ulcer, occupied the same ward as a case of low erysipelas in a wound. After doing well for a fortnight the little ulcer looked angry; sickness, high temperature, and very pinched aspect suddenly came on; on the second day his knees swelled, and on the third day he died. Pus was found in his joints. There was at the time an outbreak of phagedæna and erysipelas in the hospital generally, an account of which is given below.

It appears to me that there are probably two forms of pyæmia. One arises from systemic infection caused by some poison perceptible only in its results, and generally fatal. This is almost peculiar to hospitals or crowded patients, and the same *materies morbi* which produces phagedæna or erysipelas in one patient may produce this form of pyæmia in another, as shown by Cases 1, 3, 4, in which the original outbreak in the hospital of phagedæna was followed by a case of acute pyæmia, and that by a case of erysipelas, all occurring in different wards. The other form of pyæmia arises from the absorption of matter within the body itself, relieving itself by abscesses or secondary deposits, and only proving fatal by exhaustion when deposited in joints or when very profuse. This second form is not affected by atmospheric conditions, and occurs in private as frequently as in hospital practice. It may be greatly avoided by careful management of wounds and general attention to the condition of the patient; and this is the sole secret of the success of my friend and teacher, Mr. Callender, his constant supervision, characteristic cleanliness, and minute attention to detail preventing the possibility of any fetid discharge being left for absorption. Given a hospital with perfect sanitary arrangements and this careful mode of managing wounds, and I firmly believe that we should hear little of any form of pyæmia. Mr. Jonathan Hutchinson's conclusion, that "ventilation is of no use at all," and that "erysipelas is frequently caused by such so-called hygienic arrangements," must be a most dangerous theory for so great an authority to promulgate amongst the profession. And his conclusion that hospital air has very little to do with the causation of pyæmia, because he has seen many cases of this disease in the London Hospital and none in the Metropolitan Free Hospital, only strengthens the fact that some buildings are affected by hospitalism, while others remain free.

The following are the cases which occurred in the late outbreak in the Lincoln County Hospital in the months of November, December, and January last. The hospital was apparently in a healthy condition when the first case occurred, seven months having elapsed since the last outbreak. They are placed in the order in which they were attacked.

1. Female. No. 1 female ward. Housemaid's knee. Suppurated, was opened, and in a few days began to slough; phagedæna set in, and extended three parts round the joint; exhaustion very great. Strong nitric acid was applied; large doses of quinine did most good. Recovered in four months. She was housemaid in the hospital, but had nothing to do with the wards. The disease therefore broke out in a person who had been living in the hospital air.

2. Female. No. 1 female ward. Fatty tumour removed from back. Low form of cellulitis and great sloughing. Recovery in eight weeks.

3. Male. No. 1 male ward. Contusion over iliac glands. Counter-irritation employed; followed by rigors, sweats, swelling of knees and ankles. Rapid death. Pus found in the joints. This case was in the extremity of the building, furthest from the previous cases.

4. Male. No. 3 male ward. Contusion of elbow. Erysipelas; diffuse sloughing cellulitis; severe constitutional disturbance; rigors; very high temperature. Recovery in thirteen weeks.

5. Female. No. 3 female ward. Amputation of leg. Sloughing of flaps; rigors; secondary abscesses; very high temperature. Recovery in nine weeks.

6. Male. No. 2 male ward. Amputation of toe. Sloughing. Recovery.

7. Male. No. 3 male ward. Simple fracture of tibia and fibula. Small chronic ulcer, the size of a shilling. Sudden attack of vomiting; rigors; swelling of joints. Rapid death. Pus found in joints.

8. Male. No. 3 male ward. Periostitis. Two attacks of erysipelas. Recovery in seven weeks.

9. Male. No. 4 male ward. Small ulcer of leg. Erysipelas, with deep-seated sloughing and suppuration from glutei muscles to heel. Still in hospital.

10 & 11. Males. No. 2 male ward. Ulcers of legs. Erysipelas. Recovery.

12 & 13. Males. No. 5 male ward. Strumous abscesses. Erysipelas; cellulitis. Recovery.

14. Male. No. 2 male ward. Diseased ankle. Erysipelas; sloughing. Recovery.

15. Male. No. 4 male ward. Removal of fatty tumour. Severe erysipelas. Recovery.

16. Male. No. 2 male ward. Cancer of lip. Removal. Diffuse erysipelas; extensive inflammation of larynx; tracheotomy. Death.

17. Male. No. 5 male ward. Fistula in ano. Erysipelas. Recovery.

18. Female. No. 2 female ward. Wound of arm. Erysipelas. Recovery.

19. Male. No. 2 male ward. Wound of finger. Erysipelas. Recovery.

In the months of November, December, and January, 48 cases of open wounds were admitted; 19 of these, or 40 per cent., suffered from either phagedæna, pyæmia, or erysipelas. The cases were distributed over all the wards, and broke

out with the greatest irregularity, and the first case was that of a healthy person who had been for some time resident in the hospital,—showing that the infection pervaded the entire building. Mr. Erichsen's outbreaks in University College Hospital appear to have been confined nearly to one ward, where operations accumulated. No operations were performed in the Lincoln Hospital from the appearance of the second case of phagedæna in November until February, except the case of tracheotomy, which proved fatal. By not admitting chronic ulcers—a most noxious affection,—and by diminishing the number of patients, we have now got rid of this state of things for a time.

Lincoln.

CASE OF CIRSOID ANEURISM TREATED BY INJECTION OF PERCHLORIDE OF IRON; DEATH FROM EMBOLISM.

BY JAMES F. WEST, F.R.C.S.,
SENIOR SURGEON TO THE QUEEN'S HOSPITAL, BIRMINGHAM.

DESPITE the brilliant success which has attended the employment of coagulating injections for the treatment of nævus in the hands of MM. Broca, Gosselin, Demarquay, Professor Pitha, and others, they are evidently dangerous remedies in certain cases, especially when the tumours are about the face and head; and it is incumbent on those who have had fatal cases to publish them as a warning to others not to use such injections in the future, but to adopt some less hazardous mode of treatment. Mr. Bryant* mentions that he lost a patient from embolism a few minutes after injecting a nævus, though he does not say on what part of the body it was situated. Mr. Thomas Smith, in an excellent paper on the same subject, gives several cases in which a fatal result attended the use of perchloride-of-iron injections.† Recently Mr. Kesteven‡ has also called attention to an unsuccessful case in his practice; and although he attributes death in that instance to "spasm of the glottis, induced by mental emotion," I cannot help thinking the result was more likely due to embolism. The absence of a post-mortem examination, however, prevents the solution of the question.

Such experience as is afforded by these records and by the following case must make all thoughtful surgeons pause before recommending the injection plan of treatment, and induce them rather to have recourse either to enucleation, the galvano-cautery, the ligature, or, what I very much prefer, the wire écraseur. In THE LANCET of March 4th, 1871, I have reported several cases in which considerable nævoid growths were treated by me in this manner with success; and further experience has shown me that the écraseur is one of the best, if not *the* best means we possess for the removal of vascular tumours.

Alfred G. T——, aged nine months, was admitted into the Queen's Hospital, under my care, on June 2nd, 1873, with a nævoid tumour, about the size of a small marble, on the right ala nasi, near the tip. It had existed from birth, but was slowly growing, and beginning to involve the left ala. It was soft and compressible, and over its surface large and distended capillaries were seen in three or four spots, each as large as a pea. Deformity from the prominent situation of the tumour necessitated operative interference, and as it was considered that no mode of operation offered so good an opportunity for the prevention of an ugly cicatrix as the injection of solution of perchloride of iron, it was decided to use it. At 11 A.M., chloroform having been given, three drops were injected by a hypodermic syringe into the lowest part of the tumour. The child struggled and cried, but there was no other unfavourable symptom. Three more drops were then injected into the upper part of the tumour, and three at another highly vascular spot, when, on withdrawing the syringe, the child's face changed suddenly to a dusky hue, the hands and feet became blue, the pulse could not be felt, and the breathing seemed to be arrested; on raising the eyelids, the eyeballs were seen to be fixed and turned upwards and outwards, and the pupils were slightly dilated. The tongue was at once drawn forwards, and artificial respiration kept up for twenty minutes. During some part of this time the child breathed naturally, and then would cease to breathe until Silvester's method was again used. Until 6 o'clock the same evening he continued in a half-unconscious state, with a very feeble pulse, dilated pupils, livid hands and feet, and the angle of the mouth on the right side somewhat drawn down. About every ten or fifteen minutes he would start crying, and then relapse into his former semi-comatose state. During the evening the extremities became warmer and of a more natural colour, the breathing less laboured, and the pulse better; but the eyeballs rolled about, and the child could not be roused to take notice, and could hardly be induced to imbibe milk from his bottle. He continued in this state for three days, when he died, with well-marked symptoms of hemiplegia.

At the post-mortem examination, the membranes of the brain were found slightly congested; the anterior portion of both lobes of the cerebrum, and also the inner surface of the brain as seen in the walls of the lateral ventricles, were reduced to a soft pulp, while the posterior part of the brain-substance was in a healthy state. A clot was seen stretching from the right internal carotid artery along the middle cerebral artery of the same side. The lungs were congested. The heart contained a small quantity of dark fluid blood. The liver, kidneys, and other viscera were normal.*

That embolism was produced in this instance by the injection of the perchloride of iron does not admit of a doubt, and I therefore unhesitatingly confirm Mr. Thomas Smith's opinion that we are justified in rejecting it as a remedy for nævi on the face.

Birmingham.

* Practice of Surgery, p. 204.
† THE LANCET, Aug. 17th, 1867. ‡ Ibid., Feb. 7th, 1874.

* The notes of the case and of the post-mortem were kindly furnished me by Mr. Gilbert Smith, the house-surgeon.

Medical Societies.

PATHOLOGICAL SOCIETY OF LONDON.

TUESDAY, MARCH 17TH, 1874.

SIR WM. JENNER, PRESIDENT, IN THE CHAIR.

DISCUSSION ON CANCER.

SIR JAMES PAGET.—I regret that I was unable to be present at the last meeting of the Society, but I have had the advantage of having been able to read the careful report of Mr. De Morgan's speech at the opening discussion, published in the journals, and I probably find therein a very much more careful agreement between myself and him in reference to the pathology of cancer than I might have supposed if I had only heard the address delivered and had seen the preponderance, to all appearance, though not in reality, of the view which he held of what may be generally understood as its local origin; for I find that Mr. De Morgan holds as I do—and not I alone, but I think the large body of those engaged in the study of pathology and of surgical practice too—that we must look at the same time for a local and a constitutional origin of cancer. If Mr. De Morgan will allow me, I will read one or two sentences of his own on the matter, that they may serve as a warning to myself lest I should diverge too far. He says: "All, in fact, that we see in the life of cancer naturally leads to the belief that the disease must from the first be more than a mere local tissue change; and in one sense, and to a certain extent, I must admit that this belief is well founded." And again: "The view which I would maintain is that, though local in its origin, there is in some, possibly in all, cases a predisposition to the disease which may possibly be distributed through the system, but much more probably has its seat in some among the tissues of the body." To this doctrine I should entirely conform; and if I could find anything to discuss or anything in Mr. De Morgan's address to which I could object, it would be, not in a doctrine such as this, but rather in what I may call the only half-willing manner in which he seems to admit it—the manner in which he seems disposed to de-

preciate the belief in a constitutional origin of cancer or in a constitutional element in the origin of cancer, and to exalt more than I would the local element; for I could agree with him entirely that we must hold both a local and a constitutional element as a necessary condition in every or nearly every case of cancer that comes under our observation; and for the purposes of discussion I will admit at once that I should adopt (though from some of them I might verbally dissent) all the definitions which Mr. De Morgan has adduced—his definition of constitutional disease, his limitation in the discussion of cancer to certain well-known species of it; though I admit I should not include the rodent ulcer; I think it would be better to exclude it from the general name of cancer. Still, I think, for the purposes of discussion, we should adopt as fairly as we can the limits which the author has laid down, and not wander too far from them. If, then, we look at the pathology of cancer, which, after all, limit it as we may, is a very wide-spread disease, I think we cannot doubt that there are some cases in which it is very hard to discern anything like a constitutional origin at all. If we take those cases, though they be of rare occurrence, in which cancer occurs in scars of old burns—such scars as we see from time to time fretting and ulcerating year after year, undergoing no unusual change, and then at last becoming the seats of cancer—we are almost ready to say that there must have been so little, if any, of a constitutional predisposition to cancer in such a patient, that it required some tissue to be specially prepared through a course of years before it could either become cancerous or, in other words, could become the seat of cancer. So, too, if we see the perpetual irritation of a lip which only after years becomes the seat of cancer, it is difficult to say that there has not been hitherto such a condition as required a constitutional predisposition for the production of cancer in it. Nevertheless, even in these cases we not unfrequently observe that after passing a large number of years only as what we may call simple ulcers, or ulcers of various names not implying cancerous disease, they suddenly make a change, and become, as I would say, the seats of cancer, indicating that there may have been some conditions besides mere irritation out of which the cancer has been generated. And so, again, if we look at the regular series of morbid growths which can be traced without any apparent interval whatever from the simplest forms of fatty or fibrous tissue up to the most marked forms of cancer, it may be very hard to indicate the point at which we should maintain that a constitutional predisposition, or any constitutional element, has had to be introduced. If these, then, were the only cases to be studied, I think we might hold almost that cancer may be completely a local disease, needing, if any, only what we may regard as the lowest degree of some constitutional element for its production. But we must look to the further end of the scale before we decide that, and at the further end we find growths unlike any that are produced in the body, rapidly produced, rapidly multiplying, rapidly destroying; things so unlike anything that we can call local disease, purely and simply, that we are bound to adopt another pathology for it; or rather we are bound, as I think, to believe that in the long group of diseases which we call cancers, there may be at one end diseases in which it is hard to discern any constitutional element at all, and at the other end diseases in which the constitutional element very far predominates over anything that we can suppose to belong to locality. If, then, I now proceed to show the grounds on which I should hold the very great importance of giving a very full consideration, I will not say a dominant one, but a very full and complete consideration, to the constitutional element in the origin of cancer, it is only that I may, if possible, avert what I think may be the consequence of such a paper as Mr. De Morgan's—a paper of great weight, great clearness, and wisdom, drawn from long experience, and likely enough therefore to have great weight among those who read it. Now there are certain things in the history of cancer in which it is impossible not to discern a very large predominance of what, for brevity's sake, I will call the constitutional element; for I presume that about the existence of two elements in the origination of cancer we are all very well agreed. And the first of these is the manner of its inheritance—not the mere fact of its inheritance, but the manner in which it is inherited. And I am anxious to state that I think we cannot overestimate the importance of inheritance in the origination of cancer. The more I have seen of cancer as occurring among families whose family history is known, the larger becomes the number of cases in which well-marked inheritance can be traced. When I was engaged almost solely in hospital practice I thought it was about one in six. When I came to know something more of private practice I found I could count one in four. Now I can without difficulty count, as actual facts, not less than one in three of all patients with cancer in whose families the occurrence of cancer is well known. But this number does not nearly represent what we may very safely assume to be the predominance of inheritance in cancer. A large number of persons die of internal cancer, and convey it to their offspring, though it is never known that themselves have been the subject of cancer, or at least never recorded. A large number more die before they have transmitted the cancerous predisposition which is in themselves: for, paradoxical as it may seem, if a man have not outlived the whole age of man we are bound not to believe that he might not have been the subject of cancer; for cancer is eminently a disease of degeneracy, a disease of which the frequency increases as years increase—that is, in proportion to the number of persons living at each period of life, the number of cancers increases as age increases. So that unless a man, as I say, have lived to the full age of life, he may have died of some disease and never manifested the cancer which he yet conveys in predisposition to his offspring. The cases are very far from rare in which offspring die of cancer long before their parents. The parent lives and maintains that cancer was never known in the family, but a few years elapse and then the parent dies of the very same disease as the offspring died of, having been himself or herself quite ignorant of the conveyance of the disease of which the offspring died. I am disposed therefore to hold that it is not possible to conceive of the origin of cancer, or, I would add, of any disease of the like kind, except by inheritance. I merely state this without dwelling on it, for it is not only constitutional diseases that are inherited; local diseases are inherited with just as much surety as constitutional ones, and I mention it only that one may be able to express strongly the very great importance of studying the inheritance of cancer as indicating its origin. Local diseases are as well inherited. The ordinary fatty tumour is often by inheritance; much more commonly still the ordinary cartilaginous tumour is by inheritance; most commonly of all the ordinary cutaneous cyst of the scalp and of other parts is by inheritance. Malformations to which we cannot, with the widest stretch of the term, apply the term "constitutional" are always inherited. It is, therefore, not to the mere fact of a general or, as I would suppose, of a constant inheritance, that I would assign the constitutional condition of cancer, but to the manner of it; for the contrast between inheritance of a local disease and inheritance of a constitutional one is very striking. When a local disease is inherited it passes from progenitor to offspring in the same tissue, if not in the same place. A malformed finger may pass from parent to offspring, but a malformed finger does not generate a malformed heart or a malformed brain. A cutaneous cyst may be inherited from parent to offspring, but it is in the cutaneous tissue. We do not find cutaneous cysts growing in the stomach or testes of the offspring of those that have had them on their scalps. Fatty tumours, fibrous tumours, and cartilaginous tumours may pass by inheritance, but as they pass they pass from the fatty or the fibrous or cartilaginous tissue of the progenitor into corresponding tissues of the offspring; and it is a rule to which, I suppose, no fair exception can be made that the transmission of a local disease is of a disease from the same part, if not from the same place, in the progenitor as we find it in the offspring. Now that rule is in no sense a rule of limitation in regard to the transmission of cancer. Mr. Baker published, I think, in the Transactions of the Medico-Chirurgical Society a statistical report of the cases of transmission by inheritance of cancer amongst my manuscript notes which he had the opportunity of looking over. About half of them passed from the progenitor to the offspring as cancers and as affecting the same organ, but this is a half which affects almost entirely the uterus and the breast, respecting the tendencies of which towards cancer I would speak hereafter; but in the other half,

which are the more characteristic in this respect, there is no rule at all. The cancer of the breast in the parent is marked as cancer of the lip in the child; the cancer of the cheek in the parent becomes cancer of the bone in the child: there is in these cases absolutely no relation at all of place or of texture, and it is but fair to mention one striking instance which comes within my own knowledge of total disregard of all locality, of all tissue, in the transmission of cancer from parent to offspring. A lady died with cancer of the stomach; one of her daughters died with cancer of the stomach, another died with cancer of the breast; and, of her grandchildren, two died with cancer of the breast, two of cancer of the uterus, one of cancer of the bladder, one of cancer of the axillary glands, one of cancer of the stomach, and one of cancer of the rectum. Now it seems to me inconceivable that we can here speak of the transmission of anything local. That which was transmitted in this case must have been something which we are bound to call constitutional, and of which the locality in each person was determined by some other circumstance than that of inheritance. And herein the transmission of cancer accords with the transmission of all other hereditary constitutional diseases—with gout, with syphilis, with tuberculosis, with scrofula; for here I may mention that I think it will be well if we study the whole matter of the pathology of cancer, not by its accordance with certain verbal definitions of our own, but in its relations with those diseases which we are all of us ready to admit to be either constitutional or local. And I assume that we are all of us quite ready to admit that gout and syphilis, and tuberculosis and scrofula, are constitutional diseases. I say, then, the transmission of cancer by inheritance accords perfectly with what we see in them, not with what we see in local diseases. The gout of the parent may appear in all the possible various textures of his offspring, never, it may be, in the great toe of one of them, but in the joint of one, in the skin of another, the bronchial tubes of another, the pulmonary texture of another. Of all the variety of diseases which we class under the general name of gout, the whole may be derived from one parent who may have gout in what we regard its typical form. And I need not here speak of the varieties of form in which tuberculosis is propagated by inheritance, or scrofula, or any of those others which we regard as constitutional diseases; and this fact, I repeat, in regard to the transmission of a disease by inheritance, is so all-important in respect of what we call its origin, that I think it is to be regretted if we deviate in the least from holding that the constitutional element of the origin of cancer is one of the most important and considerable things we have to study. Then next, if we watch the effects of injury in the production of cancer, we see very notably that it follows the same rule as it does in the production of others which we call constitutional diseases. Cancer, like others that we call constitutional diseases, sometimes appears, as the expression is, of itself; there is apparently so little provocation of it that we trace nothing that we can call an exciting cause. But in a very large number of cases it follows injury, and sometimes it follows so quickly in the course of a week, or three weeks, or a month after the injury, that it is impossible to disconnect it from the changes which the injury has produced. Now we know very well, as well as we know anything in pathology, what is the limit of changes of structure which can be produced by injury in any person of sound constitution. The ordinary processes of repair, and the ordinary changes into which they may deviate without any constitutional condition being present, are as well known as are the ordinary changes of nutrition: either a complete clearing up without change of texture at all, or some inflammatory formation, some thickening, some hardening, some over-growth of some texture like to the natural textures of the body, or, at the most, the production of some tumour which increases and grows on the place of injury, but holds the character of the texture in which it is seated. And when after injury there is a deviation from these ordinary changes, we seldom or never entertain a question but that the deviation is due to some constitutional defect in the person in whom it occurs. We see a wrenched knee-joint which does not recover after the ordinary fashion, which swells and remains hot, with thickening of the synovial membranes, perhaps with the formation of abscess. We are as sure as of anything in pathology that that is because the person is scrofulous. We see an injury of a bone, and it becomes the seat of tuberculous deposit. We understand that that is because the person to whom the injury has happened is one with a constitutional tuberculosis. We see an injury done to the foot, and it becomes the seat of a gouty inflammation; of another done to joint or bone, and it becomes the seat of syphilitic disease. In all of these cases our pathology is as sure as any piece of pathology we have; it may be erroneous, but at least it is as certain as any other knowledge that we have, that deviation from the ordinary method of change after injury is because of some constitutional peculiarity in the person upon whom the injury has been inflicted. Now, it is only a reading of the same facts with altered terms which enables us to say that when a cancer comes after an injury inflicted upon a part, it is because there was a constitutional predisposition in the patient to cancer. I cannot conceive that we should have any other method of reading the fact than by assuming that here the cancerous person follows the same law as we find in so large a number of instances in other constitutional diseases, and that the ordinary consequences of the effects of injury are deviated from because of a constitutional peculiarity in the person on whom the injury has fallen. And this case is made very strong in some of those cases where injury has fallen upon persons in whom we know of cancerous inheritance. How otherwise can we read the fact, which is by no means rare, that a cancerous mother has a son whose lip is incessantly irritated, and the lip becomes cancerous. There is there no transmission of a local peculiarity, but transmission of something constitutional, which is brought out by a local injury. Therefore I would hold, as a second reason for maintaining in its fullest force the power of the constitutional element in cancer, that the production of cancer by injury is according to exactly the same rules as those by which we hold that, when the ordinary effects of injury are in any degree deviated from, it is because of some constitutional peculiarity of the person on whom the injury has been inflicted. And then next I would hold that the constitutional element in the origin of cancer is strongly marked in the constancy and in the method of its recurrence after operations—recurrence after complete excision. Mr. De Morgan speaks of the "almost constant." Well, I do not know what percentage "almost" means, but I venture to say, speaking of ordinary typical cancer of the breast, or any other part which is its most frequent seat, the number of cases in which it does not recur is not more than 1 in 500, and that exception of 1 in 500 may very well exist without any imputation on the general doctrine of the constitutional origin of cancer; for in one person in 500 it may very well be that there is only one piece of the body which can possibly become the seat of a constitutional disease, and that piece being cut out no more remains; or it may be that in one person in 500 a constitutional predisposition to the disease is, as we may say, exhausted by the production of a single growth, or a single set of germs of growth. We do not think that a disease has been less scrofulous because, after the amputation of the scrofulous limb, the person may have shown no more very marked signs of scrofula, or because after the occurrence of a syphilitic sore the appearance of syphilitic constitution may cease. However, the point is not so much here in the fact of an almost constancy—say 1 in 500 in which cancer is not reproduced after operation—as in the contrast between cancer and other tumours. For if the cases in which cancer is not reproduced in some form are not more than 1 in 500, then I would add that the ordinary non-cancerous tumours do not reappear in more than 1 case in 500. The case on the one side is as strong in opposition as the case on the other; 1 in 500 may fail in reproduction; but not more than 1 in 500 succeeds, if I may say so, in reproduction after removal. And here I must venture to say our own Transactions are liable to lead to fallacies. We get all the records of strange things, all records in which fatty tumours are reproduced, but none of the hundreds of cases in which it is not reproduced. Now suppose that these were, as in one sense they are, vivisections. Suppose a man had performed 500 vivisections to determine such a point as whether a certain mass within the body is altogether local or for the most part constitutional, and he found that after five hundred vivisections he got but one single exception to the rule that it repairs somewhere else after removal from the one part,

he would be a bold man if he repeated his vivisection; he would be surely content with that, especially if he took in contrast 500 cases more of different growths and found that they did not reappear, and he would be content to put aside the apparent exceptions for further consideration. He would not use those exceptions to overthrow the doctrine which he had founded upon the rule, a doctrine of constitutional disease which would be as safe for that as it would be for gout or tuberculosis or syphilis or any other that we are content to call constitutional. But not only is this so in regard to the mere fact of reproduction, but to the method of it. The contrast here is very striking. There are certain instances in which tumours that we must call cancerous do reappear in distant parts. These are exceedingly few. The very large majority of the so-called recurrent tumours, tumours which seem to stand in the mid-distance between the cancerous and the non-cancerous, reappear again time after time; they cannot be extirpated except by the complete removal, not merely of the tissue, but of the particular part—the whole part in which they grow. Now, this contrast is really a striking one. I once removed from a healthy, full-blooded young woman, one in whom cancer would have had its most florid course, the eighteenth tumour to which she had been subjected within the course of six or seven years. The tumours grew in the intermuscular spaces of the thigh, and we removed the eighteenth, and if she had not died of pyæmia it seemed as if there might be the thirty-eighth or the fifty-eighth—there seemed absolutely no limit to the number of times in which the recurrent tumour might be produced in the same part. If the limb had been amputated, I think it very likely she would have had no return at all; but whether it be so or not, I venture to say there is not in the whole history of the surgery of cancers a case parallel to this—not a single case in which a large cancerous mass was removed from the same part eighteen, I would venture to say eight, times without the production of similar disease in the lymphatics and in the other organs. In another case I removed a tumour for the seventh time, and then its return was destructive. A third and fourth time are frequent. Now, these things have not their likeness at all in the cases of cancer. You may cut out a little cancerous tubercle here and there from some old person—three, four, five, or six times over, but that is a different disease. You cannot find an instance of rapidly growing soft-texture vascular cancer of any form which can be removed three, four, six, eight, ten, or twenty times without recurrence, not in the place of growth alone, but in distant organs. And I believe it is vain to attempt to explain this difference of the recurrence in distant and dissimilar parts, which we find in recurrent tumours, or occasionally in the more ordinary kinds, upon any facts of difference of physical condition. I observe it is referred to the mobility of cells, to their readiness to travel, that now and then these tumours pass from one part to the other. Now, really there are cancers that multiply themselves in distant and dissimilar parts, whose physical condition looks as unfit for travelling as any that could be named. If I could name any kind of cancer which propagates itself more widely and readily than another it would be the osteoid, a mass as hard as any mass of fibrous tissue you ever found in the uterus. I know no fibrous tumour which is so hard as the fibrous mass, to say nothing of the bony structure, of an osteoid cancer, yet it propagates itself speedily and everywhere. Ordinary scirrhus cancer of the breast is at least as hard as an ordinary fibrous tumour, but the one does what the other does not—propagate itself. These recurrent fibroid or recurrent cartilaginous growths are just as soft and composed of cells and free nuclei as little held together as in any of the softest forms of cancer. They do not, except in rare cases, propagate themselves. Cancers do not fail, except in rare cases, to propagate themselves, so that I must maintain that, whichever way we look at them, the facts of the method of propagation to distant and dissimilar parts are so strong on the side of cancers that we must assume an essential difference between them and any other tumours that we might name. These are the chief grounds which impress me very strongly with the belief that we are committing a great error if we depreciate in the least degree the constitutional element in the production of cancer. I only fear lest in thus maintaining the importance of the constitutional element I should seem to have endeavoured to depreciate the local. My anxiety would rather be to hold that, in different cases, the one or the other is predominant. If I must hold that which I suppose to be the more important of the two, I should believe that the constitutional is the more important, inasmuch as it is that which seems to predominate most in the largest portion of cancers, in those therefore which we may best take as indicating the type of the disease. On these grounds, then, I should hold not unwillingly, not grudgingly, not as if I would depreciate it, the very great importance of the constitutional element in the production of cancer. But then it is said, it may be constitutional, but it is not a blood disease, and I have no right, if I assume it to be constitutional, therefore to assume that there must be a morbid material present in the blood which incorporates itself in any form in the structure of a cancer. Well, I am quite ready to admit that this is a very much less important matter than the other. The question of first importance is, whether cancer does owe its origin to a constitutional as well as to a local condition; whether that constitutional condition be a morbid material in the blood or not is an altogether different question, and does not much affect the main one. Nevertheless, I am prepared to hold that by all the analogies of other constitutional diseases, we do well if we hold that cancer also has a special material in the blood to which it owes a part, not the whole, of its origin. For if we are to hold constitutional diseases to be, as Mr. De Morgan says, and as I would say, to be all-pervading, what is it which all-pervades? There are not many things which do all-pervade the body. We have the nerves: we can hardly ascribe the constitutional origin of cancer to any morbid condition of the nervous system. That seems, to say the least, very improbable. We have the minute bloodvessels; we have, we might almost say, the organic muscular fibre; we have the connective tissue; we have the blood; and, having named those things, we are at a loss to name anything else in which the all-pervading condition can exist. We cannot talk of conditions separate from the materials in which the conditions exist. We may doubt whether it be a fluid or a germ of any degree of minuteness, but at least it must be something material; and if it be so, where is the material in which these substances all-pervade? Well, I can conceive that it might be in the all-pervading connective tissue, but it seems to me more probable, and a safer thing to hold, that it is in the all-pervading blood, because at any rate we can then hold our study by some sort of analogy with things that we recognise as being blood diseases. We have some blood diseases that we are sure of. So far as I know, we have no all-pervading connective tissue diseases of which we have any knowledge at all, so that if we hold that the constitutional condition of cancer is not in the blood, we drift away into the land of the unknowable—an excellent land to drift to, for all the most inquiring spirits would immediately proceed to find out what there is in that unknowable. But at least we have a set of diseases of which we know something as being blood diseases, and it is with them that the likenesses of cancer are found. As, for example, I presume it may be held very fairly that senile diabetes is, if not originally, yet in great part a blood disease; there is a morbid state of the blood in that disease. Except in that it effects a permanent growth instead of a temporary change of texture, the production of a cancer from supposed morbid blood is hardly more strange than production of a carbuncle out of diabetic blood. The occurrence of boils and carbuncles and other local necroses is as singular an indication of the disturbed blood of diabetes as is a cancer or any supposed disturbance of the blood that we may speak of. Uræmia, with all the local diseases it may produce, scurvy with the various diatheses which are marked by morbid conditions of the urine, these we all know to be blood diseases with appropriate local illustrations. But it is said that if it be a blood disease there are certain conditions observed in cancers which cannot be understood, and Mr. De Morgan names many of these. I have looked carefully through them, and it seems to me that the objections which are taken to the assuming that cancer is a blood disease on account of these conditions would apply with just as much force to any other blood disease that could be named. It is said, for example, that we can hardly conceive how cancer should be produced if it be in the first instance in the blood, and yet the whole condition of the cancerous person should be one of perfect health. Why

this is surely exactly the same condition as that of a man on the very eve of an outbreak of gout. Few men feel themselves fitter for either work or play than the men whose blood is so near to an explosion of gout that it would appear within a few days afterwards. In the long intervals which exist between one attack of tertiary syphilis and another there can be no reasonable imputation on the health of the person; all else in him seems sound, yet there is something at work which will again appear in syphilis. In the intervals of scrofula there is not much to be said against the health of the person, so that the fact of the general health being co-existent with cancerous manifestation in a single part is really no more an argument against the belief in a cancerous condition of blood than it is in the case of syphilis, tuberculosis, scrofula, or gout. Then, again, it is said that you observe in cancerous persons that injuries are inflicted upon them and no cancerous change ensues. Here, again, the same thing may be held. Study the same fact in the light of other constitutional diseases, and the same strangeness appears. I have seen several cases in which a day after operation the patient had an outburst of gout. I never saw a case in which the wound was hindered from healing except in so far as it might be hindered a day or two by the feverish condition which attended the gout, but the existence of gouty blood is quite consistent with perfection of repair, whether of wounds or of any other injury. Again, it is said, how can we explain the long intervals of health which exist between the removal of a cancer and the reappearance of another, if we are to assume that there is a morbid condition of the blood? Again I repeat, the same thing exactly, strange as it may seem, prevails in all other constitutional diseases. How can we explain possibly the long intervals between the successive occurrence of secondary or tertiary syphilis? How can we explain the long intervals between the outbreaks of gout or tuberculosis, or, as I would venture to say, of anything else that we call constitutional disease? And another objection is that it is inexplicable that if there be a morbid condition of the blood, cancer could not affect secondarily the parts which are its most ordinary seats primarily. Again, the same thing presents itself. Secondary syphilis does not attack the prepuce or glans penis more often than any other part. Tertiary syphilis may attack the skin, but it is more likely, or as likely, to attack a bone or joint. So that I repeat the whole of these objections which are raised, and I venture to assume that with Mr. De Morgan's learning and ingenuity he has raised all that can be raised, may be applied just as well as objections to the belief in any blood disease as to the belief that cancer may in some sense, however vaguely, be called a blood disease. And the last objection I note is, How is it that if there be the morbid blood in cancer, it should be more frequent in the female than in the male? Well, this is easily answered. Cancer is, as I have already said, eminently a disease of degenerated tissues. We are wholly misled if we suppose that cancer is a disease of healthy persons whether by local degeneracy or general; it is essentially a disease of degeneracy. As I have already said, it increases in frequency in proportion to the number of persons living as age goes on. And the apparent exception to this rule is the very exception that proves the rule, for there are in women, without any correspondences in men, two organs which enter into degeneracy, closely corresponding with the senile, at a comparatively early period of life—the breast and the uterus, and it is the predominance of cancer in the breast and uterus that alone makes the greater frequency of cancer in the female than in the male. If we separate the cases of cancer of the breast and the uterus, then cancer is more frequent in the male than in the female; but because these two organs cease to be capable of function, and enter, say at forty or fifty, or thereabouts, into a condition similar to senile degeneracy, therefore there is a rapid increase in the frequency of cancer at that particular age. But the breast and uterus of women at fifty should be compared, not with any organs of men at fifty, but with organs of men at seventy, eighty, or ninety. These organs in the women are already in a condition which one may closely compare with senile degeneracy; therefore they are more frequently the seats of the disease. It is solely this local degeneracy in women that makes them constitutionally disposed to cancer long before it would be likely to happen to men. I must maintain, therefore, that all the grounds against holding that cancer is a constitutional disease, and as also depending on a morbid condition of blood, will hold as strongly, I do not say they are altogether fallacies, but they will hold as strongly against any belief in any disease whatever being a blood disease. I am ashamed, Sir, to have occupied the Society so long, but, if you will allow me, there is one more point which I will speak to. Pathology, I think, of all studies, would be the most repulsive if it were not for some hope of utility coming out of it, and I noticed in the discussion last time, it was said our best hope of the remedy of cancer is in holding that it is a local disease. Now I venture to hold the very opposite of that. We have failed in hundreds and thousands of cases, thousands upon thousands; for years, for centuries past we have failed to cure cancer as a local disease. Every excision of cancer followed by return is a failure, even the largest and roughest attempt to cure it as a local disease. If we have any hope at all of curing cancer, it must be in the study of it as a constitutional disease; for as far as therapeutics yet have proceeded, nearly the whole power of therapeutics is that of constitutional remedies against constitutional diseases. If I look, and I do look, for a hope of recovery from cancer, it is in such a fact as this, that we have a clear remedy for syphilis—a remedy as distant in all conceivable relations from the disease of syphilis as any one part of the word is from another; and, moreover, that that remedy is of avail not merely for the existing disease of syphilis in the person first inoculated, but in the offspring. What can we conceive as passing from a male parent to a child to produce in him syphilis? Well what passes is entirely inconceivable—some material, but whatever that material may be it is alterable, in one plain sense curable, by the same remedy as would have cured it in the parent. Therefore, I repeat, if there be a hope it is in the earnest, constant study of the constitutional characters of cancer, that some day there may come to us a remedy as little expected as mercury must have been before it was introduced for syphilis, but not less sure than mercury may be the cure even of inherited disease; for mercury clearly cures the inherited disease of syphilis to a first and even to a second generation. Therefore, with many apologies for having so long occupied the Society on this occasion, I venture to hope that I may have proved that the constitutional element of cancer is so important a study that we should be exceedingly anxious in no sort to depreciate it; important not merely for the learning of the whole pathology of the disease, but even, if we may so hope, for its final remedy.

MR. ARNOTT.—I am afraid, Sir, that the eloquence of Sir James Paget has, for the present, deprived us of the pleasure of hearing many of the speakers whom we expected to hear to-night upon this important subject; therefore I, as one of the junior officers of the Society, feel that it is incumbent upon either myself or the medical secretary to fill the gap while other medical men far more fitted to speak upon the subject from their greater experience and their greater learning, are gaining time to consider what they shall say in reply (as I almost venture to trust) to Sir James Paget, or it may be in confirmation of what he has laid before us. But it has seemed to me, Sir, in listening to the wonderfully lucid speech of Sir James Paget this evening, as also in reading the no less, as it seemed to me, able address of Mr. De Morgan in the papers last week (for it was my misfortune not to hear that address) that the subject is so very vast, and the many points which have been brought before us by these eminent speakers have, some of them at all events, been so embarrassing from the almost necessary confusion between observation and hypothesis, that it is almost impossible for us to hope to gain very much more by such a debate as this than to come to some tolerably clear agreement, if it be possible, upon some one point that has been raised. And what has struck me very much this evening has been the fact which Sir James Paget pointed out, but which I confess had not struck me in reading Mr. De Morgan's paper—that those who hold apparently the local origin of cancer as distinct from its constitutional origin and nature really at the same time admit to so large an extent the part played by the constitution that Sir James Paget was able to quote, in opening his speech this evening, some sentences of Mr. De Morgan's address, and to say that he entirely agreed with those observations; and he then proceeded in his own eloquent and quite unique manner to enunciate with still greater clear-

ness how his observations, which had, I confess, seemed to me to be opposed to Mr. De Morgan, really coincided in the main with what he had laid down. I fancy therefore that one must admit in starting some great part played by the constitution in the production of cancer; but I confess that I have not been convinced (although I have been miserably shaken by what Sir James Paget has put before us) by all his arguments. We must all be grateful to him for informing us of the very much higher ratio of cases than we formerly believed in which the hereditary transmission of cancer is an undoubted fact. Hitherto, of course, I have only been able to observe hospital patients; I have therefore suffered from the drawback which Sir James Paget suffered from in his younger days, of not being able to get very clear evidences of the former family history of patients suffering from cancer, and I confess I had come to the conclusion that many of my seniors (I confess it, of course, with appropriate shame) had rather over-stated this fact—that they had made rather more of the inherited nature of cancer than the facts warranted us in accepting. But granting all that Sir James Paget has told us of the clear inheritance of cancer, it does seem to me that that does not throw very much light on the question. He himself admitted that local diseases are inherited as well as constitutional; that fatty tumours, cartilaginous tumours, and especially cutaneous cysts, were as often clearly transmitted from parent to offspring through several generations as the most virulent cancers. But he drew a very important distinction; he pointed out that it was in the manner rather than in the fact of the inheritance that the constitutional nature of cancer was specially striking. In following closely what Sir James Paget was saying upon the subject, it seemed to me that if one had time, even five minutes to consider it, and to think of the cases one had seen, and the observations one had read in the works of others, one might have met some of the remarks of Sir James Paget on this point. I think, for instance, that one explanation of the alteration in the manner of inheritance between cancer and other tumours consists in this, that the other tumours (in which group I suppose Sir James Paget would include almost only fatty, bony, and cartilaginous tumours, and some cysts which are not now generally spoken of as tumours in the sense of new cell growths) are for the most part hypertrophies of a single tissue. They are not heterologous formations; their structure is very different. For instance, the mother of a number of children has a cancer in her breast: the cancer might appear in any other part of her body; it might appear, for aught we know—although certain investigations proceeding in Germany may correct us on this point—in muscle, in bone, or in any other part of the body. Accordingly, if the mother transmit this tumour taint, this taint of scirrhus, to her offspring, it might show itself in some other part of the body. But if the mother had a fatty tumour—if she had an ordinary lipoma, she could, so far as our knowledge of pathology goes in these matters, only transmit the tumour to her offspring in such sort that the child should have lipoma in the same tissue—I do not say in the same part. I observed that Sir James Paget did not insist upon the same part being affected, although it may possibly be so in a certain number of cases. But it would be in the same tissue. So with a bony tumour, and so with a cartilaginous tumour. In another very striking part of Sir James Paget's speech he asked us pointedly why we did not find a local recurrence in cancer, as we did in those tumours which he first in England pointed out to us as recurrent tumours—tumours which we of the younger school are beginning now to call sarcoma. Here I think the histologists might come to our rescue, and help us with a ready answer. Sir James Paget said it is not anything in the physical attributes of the tumour, because what propagates itself so widely as osteoid cancer, which is one of the hardest lumps we meet with in the body in the way of new growth? What propagates itself so constantly and diffusely as scirrhus of the breast, one of the hardest tumours in the body? But it seems to me that that is hardly the point. It is not the gross hardness or softness of a tumour, although I believe that has a large share in the diffusibility of a tumour throughout the body, but it is the distribution of the elements of the tumour. In saying this I feel great diffidence, and I wish I had had time to consider what I am going to say more fully, because I am reminded at this moment that in making this observation I am also at issue with Mr. Simon, for he seemed to me to somewhat underrate the value and importance of the mobility of the elements of a tumour. If I remember rightly the report of his speech, he called it the accidental mobility of the elements of new growth. I may perhaps be permitted to illustrate what I mean by some rough sketches that I have made with reference to the point raised by Mr. Simon. When we find, as I think we do for the most part, that the mobility of the elements of a tumour stands in direct ratio to its malignancy, other conditions being the same (setting aside for a moment the fact of constitutional disposition), I think the observation is one of the very greatest importance, and we should not lightly pass it by. In this diagram, which roughly represents a section of a scirrhous carcinoma of the breast (correctly described by Sir James Paget as one of the hardest met with in the body), it will be seen that in the alveolated fibrous stroma the cells are lying perfectly loosely and detached from one another, floating in clear fluid, therefore liable at any moment to be hurried off by a lymph stream or blood stream, or through connective tissue, to other parts of the body; whereas in the other tumour, a chronic mammary tumour, in which a somewhat similar arrangement of elements is seen—viz., a fibrous stroma with included spaces filled in with cells, the cells are not epitheloid, but epithelial; they are, therefore, in close contact with one another, not easily detached and hurried away. In the cancer of infants, that cancer which is only met with in young children, glioma of the eyeball, the small round-celled sarcoma, the cells are found to be united by only a very diffluent, creamy fluid; they are, therefore, liable to be detached and hurried to distant parts, to grow widely, and insinuate themselves into the interstices surrounding the tissues; and accordingly we find that, according to one of the best monographs upon glioma recently issued from the German press, glioma is of all tumours one of the most malignant, there being only four or five cases in the whole history of gliomata that have escaped death. In spindle-cell sarcoma, the recurrent tumour, or something like it, of Sir James Paget, the cells are bound together, it is true not very firmly, by a sort of grumous intercellular material; therefore, we do not find the spindle-cell sarcoma nearly so malignant as cancer, never affecting the lymphatic glands, seldom affecting distant parts of the body. So with other tumours. Epithelioma, for instance, which is nearly as malignant in some cases as cancer, for the most part is not so malignant; and also for the most part we find that the elements are not capable of being hurried away; they are epithelial elements, and one of their chief attributes is that they stick closely together, as the large epithelial elements of healthy skin. But with regard to rodent ulcer, until the speech of Sir James Paget to-night, I had been led to regard it as a form of epithelioma, but I must now reconsider the point. In some dissections that I had made, it seemed to me that rodent ulcer was a form of epithelioma, for one sees deep down an appearance like the cells of the rete mucosum, and occasionally the birds'-nest body; the cells are more closely coherent than in epithelioma, because they resemble more the cells of the rete mucosum, not the epidermis cells; therefore they have a still lower malignancy than any ordinary epithelioma. Whereas in simple tumour, fatty tumour, fibrous tumour, there is nothing in the mobility of the cells which would tend to their reproduction in other parts or their diffusion throughout the body. Therefore it seems to me that although histologists may, as Mr. Jonathan Hutchinson seemed to indicate, have taken too prominent a part lately in the discussion of the theories of cancer, yet we have much to learn in the pathology of cancer by having regard to the minute structure of tumours rather than their gross characters. And if I may be permitted to refer for a moment to one other point, it would be this: the position of tumours affects to a large extent their malignancy. Where one cannot explain malignancy by referring to the anatomical characters, I think one may explain a great number of cases by having regard to their position. I have dwelt upon this point elsewhere, and I do not wish to be tedious by a recapitulation to-night, but I will briefly say that such observations as I have been able to make upon this point have shown me, I think with tolerable clearness, that, other things being the same, much of the malignancy of certain tumours may be explained by their position, and that in proportion as that position is subjected

to the conditions of warmth, of moisture, of great vascular supply, of free lymphatic connexions, and of occasional exalted functional activity, do we find that the tumour is likely to be malignant. In other words, epithelioma may attack the forehead at the bridge of the nose, or in the cheek, and remain there a quiet, indolent, passive sore, ulcerating its way, still making very little progress, not affecting distant parts for a period of years; but let the same disease, almost histologically the same, the same in its grosser characters, attack such a position as the frenum of the tongue, or the floor of the mouth, a part which is warm and moist, and has free vascular and lymphatic connexions, and occasional great functional activity—let it attack a part of that sort, and we have speedily, almost within a year, the death of the patient, and with too an affection of the lymphatic glands under the chin, and possibly deposits of the same epithelium in distant parts of the body. I think if we can come out of this discussion with one fact only well settled, we shall have done something. Admitting to the full that there is a constitutional something underlying the local appearances of cancer, we must admit at the same time, for Sir James Paget himself permits us to do so, that there is a similar or a somewhat analogous constitutional something underlying the simplest tumour; and if, by bearing this fact in mind, and taking into consideration with it the anatomical peculiarities of tumours, we are able to say that such a one should be malignant because of its anatomical peculiarities, or of its position, and that another will be innocent because of its anatomical peculiarities and position, then I think we need not be driven to search for any hypothesis of a blood disease in the sense of syphilis or gout.

Sir WM. GULL: I only wish, sir, that I had Sir James Paget's power of exposition; because, if I had, I think I should be able largely to demolish the charming structure which he has set before us of the constitutional origin of cancer. If you consider for a moment, and analyse the word "constitution," you will find that it has two very different meanings—a meaning with which I think we all agree, and a meaning with which we disagree. I take it that when Sir James Paget contends for the constitutional origin of syphilis from its hereditary tendency, he must refer to some condition of the ovum or spermatic fluid, or the two together, which, whether conceivable or inconceivable, exists. If we carry our minds to the very beginning of the impregnated ovum, and say that the mother or the father is to convey to this ovum a cancerous tendency, that must lie outside the blood anyhow, for there is no blood then and there; therefore the constitutional question must have relation to the constituents of the ovum at that time. Now, if we consider how this ovum develops, how (to use the technical expression, which really has no sort of meaning) it "differentiates" into the various organs of the body—that is, that they form some how or other—then, I take it, we must at last trace this constitutional condition to a local seat. The ovum has differentiated into the various textures of the body, and has been exhausted, if I may so say, in the formation of the organs. I think we are bound to admit that where the parents on one side or the other have had cancer, it is apt to appear in the offspring; yet we are bound by the laws of all scientific inquiry to believe in localisation somewhere. As the lawyers say, *latet dolus in generalibus*. Here I think is the mistake. We have got hold of a general expression in the word "constitution," which was true enough in the ovum, but ceases to be true in the adult, because there we can realise the conditions of localisation, and there we find that the ovum has differentiated into the various tissues of the body. And if there is one physical fact in the history of cancer more distinct than another, it is this—that certain of the organs are much more liable to it than others, whether it be the female breast or the os uteri. Admitting all that can be said about the constitutional origin, there are undoubtedly local seats of cancer. I challenge the constitutional theorists to tell me why it should not appear in any particular part of the intestinal tract, and why there should be certain portions of the intestinal tract which it especially and particularly affects. There are certain points which we know it will affect—say the end of the œsophagus, the pyloric end of the stomach, the duodenum; then we get over 20 feet of intestine which it does not affect except secondarily, though it is equally vascular. There are very few primary cancerous ulcers of the jejunum; they now and then occur, but so rarely that Sir James Paget puts them at 1 in 500, and perhaps they are rather more rare than that. I can only remember one case of cancer of the jejunum, and that was from contact with a cancerous part. Then we come to the lower end of the ileum, then to the cæcum, then to the sigmoid flexure. Sir Benjamin Brodie said that when you had cancerous disease of the colon it was never higher than the sigmoid flexure. I suppose he meant escaping the cæcum. The great fact still remains that cancer does show a predisposition to appear in special parts of the body. Therefore, while I fully admit the constitutional predisposition, I can only admit it when I have to do with an undifferentiated ovum. So soon as the ovum is differentiated into tissue then I say that what is called a constitutional condition becomes a local one. I cannot but think that if we could investigate the first beginning of cancerous change, and could take away the part, we should for the time rid our patient of the cancerous tendency in the particular part or place. My own belief is that when you talk of blood disease you are lost in conjecture; and when you begin to explain blood disease by reference to such a disease as gout you are still more lost in conjecture. For there is no proof whatever, as far as I know, that gout is a blood disease; nor indeed that typhoid fever is a blood disease. The blood may be a channel of disease whereby certain poisons may affect the texture; but (as I used to say when I lectured on medicine) assuming, if it were possible, a man to be all blood, he would not have typhoid fever, and he would not have gout. It is the textures which lie outside his blood that are the seat of those conditions which we call gout and typhoid fever. The blood is a very indifferent fluid; it has elements which are not indifferent—viz., the red and white cells, but quasi-blood is a singularly indifferent fluid; it has to be differentiated into a certain condition of tissue before it can assume and present the morbid phenomena of disease. Therefore, although I am not in the least able to supply all the steps of the reasoning, or adduce all the facts by which perhaps my mind would be influenced, I cannot but think that the question of constitution requires reconsideration; and that even syphilis as a constitutional disease may very much be reconsidered. Take the case of hydrophobia. I once saw a man (and the practice of medicine supplies us with many such instances) who was bitten by a mad dog, and it was thirteen years before he died of hydrophobia. Where did that poison lie? Not in his constitution, I take it—if you mean by constitution floating about anywhere and everywhere. I think it lay in the tissue that was bitten—that the poison lay there latent, producing but little effect till a certain time when some changes (this is hypothetical) came over the tissue and infected the man, and he died of the disease within thirty-six hours. I can hardly conceive it possible for that thing to have been floating about thirteen years, and then to have infected him so suddenly as to carry him off in thirty-six hours. I think it must have been lying in his tissues. Therefore I think, in dealing with malignant disease, as I have dealt with it practically, you may regard it as a local condition of the tissue then and there, however much the local development of it may have had to do with some inherited predisposition which you may properly term constitutional. I do not, then, quite partake of those feelings of hope expressed by Sir James Paget that we may succeed in finding some remedy which shall just have the same effect on cancer as that which has been produced on syphilis. It may be so, or it may not. With regard to the change that takes place, we have been told that it is a cell change. Well, perhaps it is not a cell change; perhaps it is the fluid outside the cells. Who knows? A cell is an organism of enormous extent: it is only our eyes being so feeble that makes us look upon it as a small indifferent thing. It may be infinitely more delicate than those great things which we see under the microscope, even though it be the fiftieth of an inch in diameter. I was, therefore, a little surprised at my friend Sir James Paget, when he referred to osteoid and scirrhous tumours, thinking that their hardness might be a kind of obstacle to their infecting the body so readily. Oh! those fluids, those queer things, those endosmoses and exosmoses, may act in places and parts that we know nothing about. Besides which, it is not unimportant, as Mr. Arnott has said, that it depends very much upon the place in which the tumours are situated. There are so many accidental

positions. It is notorious that affections of the bone where the veins are so large, and where there is a peculiar condition of the vein, are very prone to spread. We know that abscess in bone is constantly attended by secondary abscess over the body; so that hardness alone would not do. We must regard the accidental conditions of the situation of the part. If a man comes to us with a great lump in his side, we are obliged to tell him that we believe it is malignant disease. Of course he can only come when he gets it of sufficient magnitude for it to be palpable or for some suffering. We can make no diagnosis before, and if we could we could not touch it. Therefore the practical treatment of cancer by local operation must only refer to parts outside. In the Clinical Society I once said that if I were to become the victim of cancer, I hoped it would be in my retina, and I have the same feeling still (though the case I spoke of was, as my friend Dr. Murchison has shown, one of spindle-form sarcoma), because I should soon observe the defective vision, the eye would be quickly extracted, and I think I should have a long immunity from the disease. Certainly clinical medicine shows that cases of operation on the eye have a longer immunity from secondary cancer than cases of operation elsewhere. I think that is a highly suggestive fact.

Mr. SQUIRE.—I think the arguments of Sir Wm. Gull against cancer being a general disease are just those that tend in the opposite direction. He has referred to hydrophobia as an illustration drawn from general disease. Now I should say that the characteristic of general disease is that it affects particular organs. We know that scarlet fever will affect the kidney and the mucous membranes generally, missing the lung. Measles affects the lung, missing the kidneys. With regard to the question of blood disease and local disease, it has often occurred to me that we forget that one-half of the blood is outside the vessels, and that the fluid in the tissues is probably equal in bulk to the fluid of the blood, an interchange going on constantly between them. That consideration merges much of the differences which arise. No doubt in the case of hydrophobia the poison must have lain in the tissue for that length of time. That difference in the incubation-period points, not to any difference in the mode in which the poison acts when once it infects the circulation, but the length of time that interposes before it has got there. It was remarked by Dr. Livingstone that the bite of an animal affected him years afterwards. In another case there was a lump after the bite of an animal, and when certain suspicious but well-marked symptoms came on this lump disappeared.

Dr. PAYNE.—I should not have risen at this late hour were it not that I should wish to suggest, after the able exposition we have heard of the two different sides of this important question, that the discussion could hardly be carried much further without a somewhat clearer understanding as to the meaning of the terms which are employed. I do not think that the various speakers — Sir James Paget, Sir W. Gull, and others—have used the terms "constitutional" and "local" precisely in the same sense. Nor do I think that the term "cancer" is one which is perfectly clearly understood. I should be sorry to go, at this hour, into any lengthened discussion of the meaning of either of these words. But I presume one great element in the meaning of the word "constitutional" is this—an injury or change in which the general disposition of the body has a very large share, and the amount of external cause, injury, irritation, and so on, has a comparatively small share; but between these cases, in which the internal causes appear to account for everything, and those common diseases, such as cold, pneumonia, inflammation of all kinds, in which the external causes appear to account for everything, there are infinite gradations. That is one sense in which certainly the term might be used, and that covers both those constitutional diseases which are acquired and those which are inherited or congenital. That is to say, if a person has received into his body the poison of syphilis from any source, any injuries or irritations to which he is exposed will produce a different result from that which would be produced in the case of a healthy person; and the same if he has inherited any predisposition. Therefore that, I suppose, will cover the point which was suggested at the last meeting by Mr. Simon, and which is doubtless extremely important—viz., that cancer has, whether it is in the blood or not, great affinities with diseases such as tubercle and syphilis. He spoke, I imagine, rather of the general affinities, and in respect of the course and duration of diseases; but I would venture to suggest that there are other respects more distinctly anatomical in which these diseases may be compared, and which I think are of very great importance in their general result. But before doing this it is absolutely necessary to consider for a moment what we do mean by cancer. I imagine that by cancer has often been meant something extremely destructive, but which could not really be named until it had proved by its actual effects its destructive properties. Well, when it was found that things which had these destructive properties could sometimes be recognised at a period earlier than these destructive properties became visible, then of course it was necessary that the object should be defined by the qualities or properties which were common to it throughout the whole of its existence, and not those which became developed after a certain time. Therefore arose the necessity for an anatomical definition; and such an anatomical definition and classification of cancers and things allied to cancers I cannot but think will always be necessary. This is really not by any means a novelty (although the opposite view may have been the first), because it is precisely what was attempted at the very beginning of the century by Abernethy, who said he would classify tumours according to their structure. If people objected that that was not enough, because you could not find out what their structure was till they were removed, and also because their properties constituted a very important difference, still, he said, "All you can do is to classify things as you see them by their structure or general qualities, and then make notes of the propositions and dispositions which are attached to those structures." And that is really all that we can do now. Then I think we must have an anatomical definition of cancer. But it is fortunate to some extent that with that anatomical definition is associated a certain feature of growth. I suppose that all persons are agreed up to the present time in describing a growth as cancer when, whatever its minute character might be, it had already begun to infect or infiltrate a neighbouring part, and that is the definition which is given or accepted by Dr. Walsh in his book. Then of course it would be always clear that it could not be possible to exclude from the class things that had not already done that, because you might find, and do often find, things otherwise agreeing with cancer which have not yet arrived at that stage, but which for the most part might do so; and with most external tumours this is the case. Now, what does infection or infiltration mean? I think it has a distinct anatomical meaning, and it leads us to what is, if anything is at all, an anatomical definition of cancer. It is very clearly seen, or suggested, at all events, by the diagram which Mr. Arnott has placed on the board, and it is really this, that a change, or growth, or something which began in one tissue, has passed over from that part in which it originally began to another which is adjacent to it, and not of the same kind. For instance, the cells which Mr. Arnott represented, enclosing the alveoli of scirrhus, are, of course, as we may say, far more like epithelial gland-cells, more like cells of the gland near which they arose, than are the bodies normally found in connective tissue, whether they are precisely like gland-cells or not. And so much is that recognised, that almost everyone in describing cancers of that kind agrees that they have something of a glandular character—that is, that parts which were not originally like glands nevertheless have become to a certain extent like them—that is to say, that quality or mode of growth, or something which originally belonged to one tissue, has passed over to another. And I imagine that anatomically one can give no better definition of cancer than that there is a kind of duality in its structure, that one part of the structure, looked at anatomically, is of one kind, and another part is at all events different, whether actually of another kind or not—another kind essentially. At all events, it is different in some respects. Now, let us consider what this simple fact, as Mr. Arnott pointed out, means. It means this, that when any disease or change has once bridged over that very narrow but very important interval which separates tissues of one kind from tissues of another kind, it is almost as good as if it had already spread all over the body, because there is no longer in the whole course of its transmission through the body any barrier nearly so complete and incapable of being crossed as that which it has already passed.

That explains the old characters which are so much relied upon by surgeons, of adhesion of the skin, and fifty things more of that kind. It means that something which did not begin in the skin had got to the skin, and having got to the skin it might go anywhere else. Therefore I think this original property of infection, which includes essentially the dispersion all over the body, is in one sense an anatomical fact. Of this anatomical fact, this passing over, this morbid change from one part of the tissue to another adjacent tissue, there are many explanations. It has been explained as a matter of simple growth. It is explained in the case of epithelioma, I confess with a great deal of reasonableness, as a matter of growth: that the skin leaves its proper site and grows inward. What makes it to do so we do not know. The same explanation is extended to other forms of growth. There, perhaps, we lose ground. At all events, we may say that it is at present less clearly established that this transference of growth does take place in the case of other tumours. Then there is another explanation—namely, that growth in one kind of tissue infects or causes a different kind of growth in the adjacent tissue; and that which, I suppose, is to some extent regarded as the opinion of Virchow, who talks so much of epithelial infection, is essentially the same as was suggested at the last meeting by Mr. Simon, who said he thought the virus, or poison, or material, whatever it was, which stirred up cancerous growth, had something in it analogous to spermatic elements, and acted, not as a simple stimulus, but as a physiological stimulus; only he imagined that this did not necessarily reside in or was not always attached to cells, although it might originate in them, but might be distributed throughout the body independently of those. Allied to this is another opinion, which is exceedingly plausible, but has very little solid foundation at present—that is, that there is a conjugation also comparable to genesis. Now in all these views my question might imply something much more delicate than cell life—something coming out of a cell, but infinitely smaller and more refined. Then, however, it becomes a very important question whether any positive evidence of very minute anatomy of growths and so-forth bears out any of these views. If we could really detect, microscopically or chemically, something material, and call it cancer, and put it in a test-tube and get it under a microscope, it would be a comparatively easy task. That has not been done yet, but it has been pretty nearly done in respect of tubercle. In a great number of cases it is clear, whatever the material is that stirs up the new growth, that it is transmitted through the body in attachment to cells. If this is the case, we have here a constitutional theory of cancer which deserves that name at least as much as tubercle and as much as syphilis, in which something is introduced from without. At the same time, it is something which is so far material that it is always attached to local growth, and therefore is, we will say, essentially local. It seems to me that in this sense it might bear both terms, constitutional and local. Whether this material could ever exist in the body in any diffuse form of course is at present to us unknown. I do think that to some extent there may be a combination of the two views. With regard to the hereditary condition, we owe a great deal to Sir James Paget for having separated the common argument drawn from inheritance, and given it a far more definite form. Commonly it is urged that cancer must be constitutional because it is hereditary, whereas he has shown us that it must be constitutional, if constitutional at all, because it is hereditary, only because it is hereditary in a particular way. Then what is inherited must surely be, not anything absolutely separable from a body, and therefore in the nature of a poison, but something with some property of tissue. I do not wish to add anything to what has been said upon that point, but I would suggest that the argument which Sir James Paget used about the disease of the uterus and mamma—namely, that these are organs which in the female sex are subject to degeneration at a comparatively early period, of the same kind as that which the whole body undergoes in the male at a somewhat later period—is surely in favour of the predisposing cause of cancer being a condition and property of tissue rather than anything else in the whole body, either a property of the body as a whole or a property of the blood, or anything else, because it is tissue that is changed. I venture to request on behalf of those concerned in histological inquiries a little more indulgence than we have received from some speakers in regard to the inquiry whether we cannot define objects by the properties they present at all periods of their career. And if these characters are extremely minute, that surely is rather the misfortune of histologists than their fault; but until we have a precise enumeration of all the minute characters of tumours, it will not be possible to construct text-books and works of reference in such a way as that we shall know precisely to what we are referring.

The discussion was then adjourned.

CLINICAL SOCIETY OF LONDON.

FRIDAY, MARCH 13TH, 1874.

MR. PRESCOTT HEWETT, PRESIDENT, IN THE CHAIR.

DISCUSSION ON PYÆMIA.

THE adjourned debate on Pyæmia was resumed.

Mr. SPENCER WELLS.—The difficulty, Sir, that I have in entering upon this discussion arises from the extreme amount of material that is thrown before us in your paper and in the different discussions which have been held upon it. I have been looking them through in the very admirable reports that have been given in the medical journals, and it seems to me there are so many points for discussion that the difficulty is in selecting any one of them which one could bring before the Society in any reasonable time. If, however, I confine myself to the concluding sentence of your paper, in which you ask the Society to investigate the causes of pyæmia—and I suppose by that you would mean the causes of pyæmia in private practice particularly,—the subject will be narrowed, and one may, in the few minutes at one's disposal, endeavour to throw a little light upon it. If we look upon pyæmia in the same light, the causes of the disease must be pretty much the same either in hospital or in private; and the only difference would be in which the disease is the most frequent. There is just one question which I hope you will answer when you come to reply—namely, the sort of proportion that the twenty-one cases in your paper bear to the number of cases which have occurred in your hospital practice during that time. It seems to me, indeed, that the number of cases is very considerable, even during the twenty or thirty years of your practice which they have run over; and the information I should like to obtain is, whether, during that time, in St. George's Hospital, there has not been a very much larger proportion of cases of pyæmia. So far as my own experience goes, looking back to the very commencement of my professional life, I should say that it is only in crowded hospitals that I have ever seen the disease. In St. Thomas's Hospital during my student days one had hardly begun to recognise it. Erysipelas was very rife, but I really saw nothing of pyæmia until years afterwards at Palermo, when the hospitals there were crowded after the bombardment, and again in Paris in 1848, where I was sent by the Medical Department of the Navy to report upon the gunshot wounds. There one saw the disease every day in the crowded hospitals, where the wards were filled with surgeons and soldiers who had been wounded at the barricades during the fight under Cavaignac. Previously to that, from 1841 to 1848, I had been in a very large naval hospital in Malta. The number of patients was considerable, but we had enormous wards—they were like churches—and although there were some forty or fifty patients in each ward, the breathing space was so ample that I do not believe there was a single case of pyæmia during the whole of the six years that I served there. I cannot recall a single case. I have seen cases of erysipelas come in from ships, but with care in isolating those patients, the erysipelas did not spread. When I went to Paris in 1848, and got into the crowded hospitals, I saw the disease every day. I come to London and see no pyæmia at all. Then I go to the Crimea, and the very first thing I meet with in the hospital at Smyrna is pyæmia. The wards were crowded; patients were there with frost-bites, and a few with gunshot wounds, and on the least disturbance of any of the sloughing tissues about their feet or hands, pyæmia came on, and all the patients died, just as they die here in London in some of the

hospitals that are crowded and are placed under conditions that might be improved. Then, Sir, returning from these two spheres where pyæmia was observed, I practised in London a good deal in private, and partly in a small hospital, and in these two situations I have performed a very large number of operations in which the patient is supposed to be extremely susceptible of any poisonous influence. Various speakers in this discussion have pointed to ovariotomy as an operation which cannot be performed in hospital wards with an ordinary chance of success, or as an operation which is much more likely to be followed by some poisoning influence upon the patient than almost any other operation in surgery. I have now performed more than six hundred of these operations, and a great many other cases in which the abdominal cavity has been opened, and a certain number of other operations during the last fifteen years, but neither in private practice nor in the hospital have I seen one fatal case of pyæmia. Saying that, I must just explain for a moment what I mean by pyæmia. I mean by it the disease which we know by recurrent chills, by high temperature, by profuse sweatings, followed by deposits of pus in some joint or in cellular tissue or some internal organ. Taking that as what I mean by pyæmia, and what I fancy is pretty generally meant by it, I have never seen one fatal case either in the hospital or in my private practice, while the only cases that I could call pyæmia that I have seen have been slight cases of feverishness following operations, with swelling of some joint, called rheumatism; with some little hurry of breathing, probably accounted for by some pneumonia, or some pleurisy; or with some little jaundice, accounted for possibly by some congestion of liver; but the patients getting well. With the exception of those slight cases, I may say that pyæmia has been to me quite unknown, both in the hospital and in private. What I have known other people call pyæmia is a condition which I should call septicæmia—a condition in which, a cyst being emptied of its contents and air admitted, the contents left in the cyst, or contents which have formed afterwards, have putrefied, and then the patient has been poisoned by this decomposing fluid. That condition I have frequently seen, ending sometimes in death and sometimes in recovery; generally ending in recovery if the cyst containing the putrefying fluid was removed early enough, frequently ending in death if it was only drained, or if it was imperfectly drained, or simply washed out by a continuous injection of some antiseptic—iodine or carbolic acid. Under any of these modes of treatment death has generally followed, but there has been no secondary deposit of pus, no pus in joints, no pus in internal organs; therefore, I do not believe that could correctly be called a case of pyæmia at all. But I think we might safely say any mode of preventive treatment, or any kind of caution which would prevent patients from suffering from septicæmia, might also fairly be looked upon as likely to protect them from pyæmia, and all the difference that I can see between the probable causes of septicæmia or pyæmia in hospital practice, or in private practice, is the greater tendency in hospital practice, or the greater liability of the patient to contagion. It seems to me you have very much the same conditions to guard against, and the patient is very much under the same conditions, whether he is in a small room with one or two people—a patient, for instance, in a small room in a lodging-house in London, with a nurse and some one of his relatives, three people together in a small room with imperfect ventilation,—or whether he is in a ward of a hospital, where there are twenty or thirty patients all crowded together, with a certain number of nurses; with this exception, that in the one case you have a contagious influence bearing upon your patient, and in the other you have not. It seems to me that your private patient is guarded more or less from contagion, to which the hospital patient is subject. Therefore, the question of prevention bears mainly upon the question of contagion or infection, or its absence; that you must, when you are searching for the causes of pyæmia, and endeavouring to avoid them, in the first place put aside everything that is likely to expose the patient to the inoculation of the poison or its reception through the breathing organs, and then you have to look to all the conditions about the patient which would render him liable to receive the poison. If he is breathing pure air, I suppose his blood is likely to be in a much better state, and he is much more likely to throw off any injurious influence to which he is exposed than a patient who has been depressed by bad air or by imperfect clothing, exposure to cold, insufficient or bad food or drink, or any other cause which may put a patient into a state of health in which he is less likely to throw off any poison or the effects of any injurious influence to which he may be subjected. The point which Mr. Callender raised, and which I think is of very great interest in this matter, particularly with regard to the causes and the prevention of pyæmia, is that by very great care in the treatment of wounds, by maintaining the wounds in a state of perfect rest, by maintaining the most scrupulous cleanliness, by avoiding anything that can possibly injure the patient, locally or generally, you may obtain in a hospital just as good results as can be obtained in private practice, and may have pyæmia under control. In that I most fully agree with him. I do believe that although you may occasionally find pyæmia arising in the most healthy country house, in the best situation, as a rule the disease is one which is under our control, which ought to be, and which will be prevented when we have obtained in our hospitals as good general conditions as we have in private houses. When you can put a patient in a hospital in the same condition as one who is in a private house, with all the comforts of life about him, and protect him from contagion and infection, then I believe you will get just as good results in the hospitals as you do in private practice, and pyæmia will be no more rife in the one than in the other. If this discussion tends to spread that belief amongst us, I believe it will lead to a much less mortality following operations than exists now, and that there will be no such thing as what may be called cases of excessive mortality in operation. I hope that will be the result, and that we shall avoid the error which we are apt to fall into of people saying, "Oh, it does not matter. You do an operation; you cannot tell whether the patient is going to get well or not. You may do a slight operation and the patient will die, you may do a serious operation and he will get well, and you do not know why it is so." I trust that we shall be led by this discussion and by your paper to examine more carefully into the causes of excessive mortality after operations, and determine to avoid them, exercising the most rigid self-examination whenever a patient dies that ought not to die. If you do a serious operation attended necessarily with great risk to life, and the patient dies, nobody is to be blamed; but when after some trivial thing, some little operation of no moment, the patient is cut off, and cut off by some causes which might possibly be prevented, then I say the most rigid self-examination should be made by the surgeon, and he ought to go into every circumstance surrounding the patient—everything about the operation, everything about himself and his assistants; and I believe if that is done, and every case is carefully examined into, some cause or other will be discovered. If that is the result of this paper, I think you will have to be congratulated most sincerely upon having brought the subject before the Society; your example will be handed down to others, and the good you have done yourself will be multiplied exceedingly by those who follow you.

Dr. GORDON.—I have had the honour of being invited to this most interesting discussion. I feel that I am very imperfectly able to take part in it, but it has been my lot to see a good deal of pyæmia under peculiar circumstances, and I think and act upon the feeling that it is the duty of everyone who has had any personal observations in such an important disease to contribute the result of those observations for the benefit of the profession. I may observe that I am totally unaccustomed to appear before such a Society as the present, and I trust that any shortcomings in my expressions may find some indulgence. My career hitherto has been rather one of action than of speaking. I had the honour, as perhaps some of you may be aware, on the outbreak of the Franco-German war, to be sent to the French army as medical commissioner from the War Office. In that capacity I was, through the chances of war, besieged in Paris, and I had many opportunities, through the great courtesy of the French medical men in military service, of seeing the wounded on and from the field of battle, upon a scale such as I hope I may never be called upon to witness again; and I also had many opportunities of seeing the terrible ravages committed amongst those wounded men by pyæmia and allied diseases during the time that they were under treatment in the ambulances and in the hos-

pitals. Those hospital diseases by which the mortality was chiefly occasioned included hospital gangrene and erysipelas, although both were in a much smaller degree than seems to have prevailed, according to the accounts of the old medical officers who served in the Peninsular army. Pyæmia prevailed quite to as great an extent, I should think, as it did during the Peninsular or any other war. I may mention, with reference to the observations made by a previous speaker with regard to the clear distinction between pyæmia and septicæmia, that we saw a number of cases in the hospitals in Paris, some of which were described as pyæmia and others as septicæmia; and I must say that, although others no doubt better qualified than myself were able to draw distinctions between those cases at the bedside, I very often failed to do so. It seemed to me very much that the one was simply a stage or form of the other. I may observe that, since this discussion commenced, I have referred to the works of the French authors on the subject, and I find that, whereas one set described septicæmia as being an acute and very fatal form of pyæmia, the patients seldom if ever recovering, another set (and among them, if I mistake not, M. Legouest) describe it as a chronic form of pyæmia from which recoveries are very frequent. I mention this point as indicating that, according to my view, the distinction is not yet completely drawn between the two. There have been many discussions in Paris as to the particular poison by which pyæmia and septicæmia are respectively occasioned, and I have no doubt that you, Sir, and others present have read the discussions which took place upon this very subject some time ago, shortly after the war, before the Academy of Medicine. If I mistake not, it was M. Verneuil who described the active principle of septicæmia as being capable, not only of being collected, but of being tested. He described it as an alkaloid. He further described it as capable of being converted into a sulphate, and the solution of that sulphate, when inoculated, as being capable of producing the disease. I mention these things in order to support the view that I have just expressed—that in many cases I was unable to draw a distinction between the two diseases further than that I believe the one to be a modification and a stage of the other. I may observe, also, with reference to the statements that have been made before the Society regarding the prevalence of pyæmia in civil life, that I have been associated more or less intimately with military hospitals and with soldiers for a period of thirty-three years, during which period, except in a time of war, I have never had occasion to see a single case of pyæmia in a military hospital. I believe that nothing can more testify to the success and efficiency with which sanitation is carried out in military hospitals and barracks than that fact when compared and taken in connexion with what we have heard with regard to the occurrence of the affection in ordinary civil life. The period at which pyæmia usually happened, and the class of cases in which it usually happened during the late war, were these:—As a rule, it followed more particularly severe injuries—such as shell wounds affecting the large bones, and joints when implicating the heads of the bones. In these cases the symptoms usually showed themselves from the fourteenth to the sixteenth day after the receipt of the wound. It was very seldom indeed, whatever method of treatment was followed, that the symptoms appeared before that date. After the manner in which the last speaker has described the symptoms, I need not again refer to them. I will only say that the symptoms, as observed in Paris, were precisely those that have been already mentioned, but, in many instances, in an extremely aggravated degree—that is to say, that the first chill, instead of being an ordinary shiver, was sometimes extremely intense. The succeeding fever was equally intense, the temperature frequently rising to 104°. The state of perspiration was so intense that very frequently the whole bed became so completely wet as to appear soaked. There is one symptom, however, that I may mention that has been alluded to in this discussion—that is, the peculiar odour arising from the perspiration in these cases. I have seen and heard it described as an odour similar to that of malt. To me it is anything but like the odour of malt, which is more or less sweet and agreeable, while the odour arising from pyæmia—at all events, such pyæmia as I have seen—is extremely sickening and heavy, and of a kind that when once perceived can never be mistaken. Another symptom that I may mention is the peculiar aspect of the countenance. Immediately on the occurrence of the attack the countenance becomes anxious, sharp, and desponding—the whole expression is that of despair; the mind very soon becomes disturbed. But I have seen many instances in which, after considerable mental disturbance and delirium, the patient seemed to recover his ordinary senses for some time before death. But a remarkable indication was the extreme despondency with which all patients attacked with this terrible disease were seized immediately on the symptoms displaying themselves. Another indication was the attack of local pain. Sooner or later, usually after the first attack, local pains manifested themselves, sometimes in the form of arthritis, always ending with purulent deposit, and sometimes in the form of pneumonia and bronchitis; and I have reason to believe that many deaths in Paris that absolutely depended upon pyæmia were returned officially as arising from those diseases. Therefore I think that in such instances either the precise nature of the cases could not have been apprehended, or probably there might have been other reasons for returning them otherwise than they were. I may also observe that the period during which life was prolonged after the first attack of pyæmia was, as a rule, from four to ten days. It was very seldom indeed that pure pyæmia permitted the patient to survive beyond the tenth day. Instances were seen and recorded in which life was prolonged to the twentieth day, but these were extremely rare. As to the appearances after death, I have no doubt that they are extremely familiar to all here. Besides, when I was in Paris I really avoided very much going into a dead-house. In the first place, my duty lay chiefly with the living, and in the second place, the scenes in the dead-houses on the few occasions on which I entered them during the siege, and when great mortality was prevailing, were such as to prevent any person who was not absolutely obliged to go from entering them. With regard to the treatment and the question of recovery, I find, according to the reports of the meetings of this Society, that the recoveries are recorded as being not by any means infrequent. As I have remarked in the work that I published on the War, so far as I remember, I have never seen a case of pyæmia, as it occurred in Paris, recover. And on reading the work on Military Surgery by Legouest, I find that he only records two in the whole of his experience, and he is a very old French army medical officer. So that the chances of recovery from pyæmia, when it has fairly commenced in a state of war (I say nothing with regard to ordinary civil practice in times of peace), are very small indeed. There were several kinds of treatment employed. One was—and I believe it is that which was adopted most frequently—the administration of very large doses of quinine. Various disinfecting agents were applied to the wounds; bisulphates, such as the bisulphate of soda, were also employed, but, I believe, not followed by any success. M. Demarquay, whom I followed a good deal during the siege, in order to prevent the occurrence of pyæmia, was in the habit of spreading a chlorinated tincture of iron over the raw surfaces, but I am afraid that the circumstances conducive to pyæmia were so very powerful that the application of his remedy had very little effect in preventing them. There was one important measure that I think ought always to be adopted in such cases, but, unfortunately, on such a large scale as we saw in Paris, it was impossible to carry it into effect—that is, immediately on the occurrence of a case of pyæmia, you isolate the patient. In some instances this was done; but after the great battles the wounded came in and had to be admitted on such an enormous scale that it was perfectly impracticable to isolate even a tithe of them. As to the predisposing causes of the affection as witnessed in Paris, of course the usual conditions of war were the most formidable. These conditions included a great and constant fatigue; want of sleep at night; continual exposure to the weather; irregular, badly-cooked, and insufficient food. Then above this was the loss of *morale* by want of success; and I believe it is the experience of all military surgeons that these diseases are always found to be more prevalent in an unsuccessful than in a successful army. This question of want of *morale*, and the despondency arising from it, became a matter of particular attention by one of the operating surgeons, M. Gosselin. In all cases admitted into La Charité he took the greatest possible trouble to raise the confidence of the men. He ap-

pealed to them and to their friends to encourage them in every possible way, and I believe in many instances with very good result. Another point has arisen as to whether this is a hospital disease or not. Now in Paris we found that, according to the number of severe cases in a hospital, the disease seemed to increase. It oscillated, in fact, according to the number of severe cases in the hospital—that is to say, soon after each of the great battles, in a period of fourteen or fifteen days, pyæmia became extremely prevalent. And after the number had been thinned—as, unfortunately, was frequently the case—the relative prevalence of pyæmia became less. In some ambulances, again, in which only trivial cases were admitted, the relative prevalence of pyæmia was extremely small; so that I take it to be that in hospitals the chances of patients becoming attacked with pyæmia are in relative proportion to the number of severe cases, either in the ward or in the building. In that way it seems to me clearly to be depending upon hospital conditions. I may also say that the fatality of the wounded, not only from pyæmia, but from the other diseases that I have alluded to, increased progressively during the siege. After the first battle that took place round Paris on the 21st of October, the success with which the wounded were treated was something so remarkable that we were led to believe that almost any case would recover. However, as the fatigues connected with the campaign, the exposure, the bad food, and other causes contributed their quota to lower the general system, the mortality became greater. In November it became very considerable. After the battle of the 21st of December it was very great indeed. After the last battle on the 19th of January, 1871, the mortality was so great that it seemed that almost every person who had a severe injury died. It was also remarkable in the aspect of the wound that reaction seemed never fairly to set in. There was sanious discharge from the time of the admission of the patients. Their vital powers gave way, and they seemed to sink. Among active causes of the disease noted at the time, one was the great loss of blood to which the soldiers, already debilitated by the causes I have mentioned, were exposed. Many of them were left—and it could not be avoided—for hours on the field of battle before they could be attended to. Many others, in consequence of the immense crowd of the conveyances by which they were brought into Paris blocking up the roads, were detained for hours before they could be brought into the hospital; and when they were brought in, the surgeons were comparatively few, the attendants still fewer, and sometimes not well trained; and the result was that, from these and other causes, very great delay sometimes took place in performing the necessary dressings. Then, again, the patients, as they were brought in, had to remain in the clothes in which they had been wounded, many of them completely soiled with the discharges of blood and other matters, all of which began to decompose by the time they were brought into the hospital. In many instances the clothes, after being taken off, instead of being immediately removed from the ward, were allowed to remain hanging about the walls. Cleanliness was neglected in the wards, and in that way, I believe, a great deal of pyæmia was caused. These circumstances arose from no fault of the medical officers or the private medical men. The hospitals were not under their superintendence. They had only to treat particular patients in particular beds, and the general arrangements were beyond their control altogether. Another cause, I think, was a very important one. In dressing the wounds after the patients were admitted into the hospital, sufficient attention was not paid to the necessity of removing them from one bed to another. The bedding became more or less saturated, if not with actual discharges, at any rate with emanations, and I believe that many cases of pyæmia were thus introduced. Although the bedding was apparently clean, the whole thing had not been changed, and thus pyæmia was occasioned. Professor Moesetig, of Vienna, who had charge of the ambulance of the Corps Législatif, adopted the plan at a very early period, and carried it on throughout the siege, of having two beds for every wounded man. Whatever the nature of the wound, it was dressed at least once a day, and sometimes twice. Every man once in twenty-four hours was thoroughly cleansed, bathed, and sponged, the whole of his dressings were changed, and he was moved on to a completely fresh, clean bed, and I believe that had a very considerable effect in diminishing the mortality. Another plan of treatment which I believe had also a considerable effect in modifying or preventing the occurrence of pyæmia was that adopted in the American ambulance—namely, the employment of oakum in the treatment of wounds, especially of the lower extremity, wounds of such a nature as prevented the patient being in a recumbent position. Oakum was employed so as to completely surround in a thick layer the whole of the wounded limb. All discharges soaked into it, and in that way it seemed to me to prevent the decomposition of the discharges, and at the same time kept the wounds clean. In contradistinction to that, I may mention the French system of treating wounds. I believe many were in the habit of putting on too many folds of cloth. First there was the charpie, over that cotton, then compresses, and then bandages. Immediately these were taken off, the emanations from them showed what was going on.

DR. BURDON-SANDERSON.—I may say, Sir, with perfect truth, that when I first came to this discussion I made up my mind in the most determined manner that I would not take any part in it, because my whole object was to listen to the results of the experience of surgeons as to the very important question which you, Sir, have brought before the Clinical Society—viz., the origin of hospitalism or pyæmia from external causes. But inasmuch as it is extremely difficult, when the causation of any disease is discussed, not to get on the other subject, the question of its nature, it is natural enough that that point should have been brought before the Society by various speakers; and this constitutes my excuse, not for speaking at any length upon the nature of pyæmia, but for taking that as a starting-point. If I were asked the question straightforwardly and categorically, what do I think is the nature of pyæmia, I should certainly refuse to answer; or, at all events, I should content myself with placing before my interrogator the opposing opinions which are entertained upon the subject, and leave it to him to choose which of them he preferred. But if the question were asked by someone who really desired information, as, for example, by an intelligent student, I should certainly not give him the husks of opposing opinions. I should endeavour to explain to him that although we know very little indeed about pyæmia, and should like to know a great deal more, yet we do know something. There are, I think, some certainties at which we may arrive with reference to pyæmia. In the first place, I think it is a certainty, and has been shown to be such during the progress of the present discussion, that there is such a thing as hospitalism. To deny it, would be to deny the whole experience of surgical hospitals, and more particularly of hospitals devoted to puerperal cases. It is altogether impossible to disregard this evidence; therefore, we may put this down as the first certainty with reference to the disease—a certainty which I am sure there is no one in this room disposed to question. Then the next certainty is that pyæmia is not always dependent upon external causes. The evidence of this is to be found in the cases which you, Sir, have brought before the Society; and there are a number of other cases, more familiar perhaps to physicians than to surgeons, which very much strengthen this position. In the first place, there is the category of cases to which Dr. Stewart referred in the course of one of the discussions, resulting from typhus fever cases, which occur in the period of what would otherwise be convalescence. I do not, of course, refer to cases in which there is diffuse cellulitis, as, for example, in the neighbourhod of the parotid gland, often occurring after typhus fever, but rather to cases of actual pyæmia—cases characterised by all the symptoms of pyæmia, and usually associated with pyæmic arthritis. These constitute one series, and they lead on to another set of cases which are also tolerably well known to physicians—cases of pyæmia of the most exquisite character, having both the local and constitutional conditions of pyæmia, and resulting from acute necrosis of various kinds. Of course, the most common of these cases are those which arise from necrosis of the temporal bone—cases which run a chronic course for a certain time, and afterwards symptoms of pyæmia break out—cases which are characterised again by pyæmic arthritis, and are accompanied by the characteristic off-and-on fever of pyæmia, alternate shiverings and high temperature; in fact, presenting the characteristics of the disease in its most exquisite form. Of course, the cases associated with necrosis of the temporal bone might possibly be supposed to

be of external origin. The suspicion might arise in the case of the temporal bone that infection had been introduced into the primarily affected parts from without, but this certainly could not apply to the many other cases in which the disease commences from necrosis of other bones. Many such cases as these are on record in which there is not even the possibility that the disease can have arisen from without. Well, by the combination of these two certainties, one comes to this conclusion, that one must not regard the question of the external origin of pyæmia, the influence of external circumstances in producing pyæmia, as an alternative question. It cannot be regarded as a question on which one must either entertain one opinion or the contrary. We must admit both. We must admit the influence of hospitalism, and of external conditions generally; and on the other hand, we must admit that the whole process of pyæmia can originate in the organism independently of external influences. That is completely supported by the results of experimental pathology, it being well known that the whole series of pyæmic phenomena may be induced in animals under conditions which absolutely exclude the influence either of atmospheric or other external causes. Then I think another certainty which we may arrive at with reference to pyæmia is this, that it is a process which has a beginning and a termination of a definite kind; that is to say, that in every pyæmic process you may distinguish a focus, a centre of origin, lines of diffusion, or of distribution, and secondary results from that distribution. In every case you have an initial process in which infection commences, from which that infection spreads, and secondary processes which arise out of this primary one. Well, this at once suggests important questions, and perhaps the most important and fundamental questions in the pathology of pyæmia. In the first place we have to consider what is the primary process—what goes on at the focus or foci (because of course you may have any number of foci); and, secondly, how the diffusion or distribution of the infective material takes place. As regards what goes on at the focus, I think there is one point we may assume to be tolerably certain, and which everyone would admit,—that you must have there the process of inflammation. And if that is the case, the next question that arises is, what is there special in this?—because we know that processes of inflammation which are perfectly similar to each other in their general characters may in one case prove to be infective—that is, may give rise to general symptoms and secondary inflammations elsewhere,—and other inflammations, other local processes similar to them, may not be so; and consequently there must be some special character in a local inflammation which gives to it the power of infecting the system. One knows what answer would be given to this question by many pathologists, and by several who have spoken during this discussion. They would say at once, it is the septic character of the inflammatory process, it is the destructive character, the necrotic character of the process. Well, fact and observation do not support this conclusion, neither the facts of clinical experience I think, nor certainly the facts of experimental pathology. In the first place we know that purely septic products never produce the intense irritation which is produced by pyæmic products, or by the products of pyæmic inflammation; and, on the other hand, we know, what I think is a striking enough fact, that all pyæmic products lose their irritative property—their property of exciting the tissues to inflammation—by being kept. That is to say, a liquid which, in the first instance, when it is perfectly fresh, is extremely irritant, and cannot be brought into contact with living tissue without exciting a most intense reaction, after a few hours—that is, as soon as the septic process has commenced—gradually loses that intensity of action, and passes into much feebler kind of action, which one may truly recognise as septic. Consequently we may entirely separate from each other these two modes of action, the septic action and the action which is dependent upon the peculiar inflammations which are infective. But, at the same time, we must admit that there is no pyæmic action which has not also septic qualities. It is now well established, I think, that in every pyæmic inflammation, whether it be a primary or a secondary one, in every focus of pyæmic action, you have always the presence of septic products. Then the other question is, how these products are diffused; and here again we come across the opinions which have been expressed by one or two speakers. The ordinary surgical answer to the question would certainly be—"I believe, at present, that it takes place through the venous system; that thrombosis of the veins with phlebitis and the formation of emboli in the venous system are the necessary conditions to the completion of the pyæmic process." Well, there are a great many reasons why pathologists have now extended the meaning of pyæmia far beyond the limits of those cases which are characterised by the fact of embolism on the one hand, and the fact of phlebitis on the other. In the first place, we know that emboli may be introduced into the circulation without producing phlebitis—may lodge in the tissues and remain there and not produce any results, even though embolisms consist of fibrine. Secondly, we know that substances which are not large enough to be embolic at all, which are not of sufficient size to plug any vessel, yet produce pyæmic results. And, further, we have many examples which show that in many cases of pyæmia, in which all the characteristic symptoms of the process are present, there is no affection of the veins of this nature; that, in fact, the channel of communication is the cellular tissue. So all pathologists at the present time, I think, are agreed in believing that the channel of conveyance from the first focus to the secondary foci, and from each secondary focus to the other foci (which to them are secondary), may be either the lymphatic system or the veins. We must not assume the one or the other to be the exclusive characteristic of pyæmia. The practical result of all this is that we really do know very little about the process, but that one or two things we know with tolerable certainty. Then as regards its bearing upon the practical question, not only of the causation, but of the management of pyæmia, it strongly supports the desirableness of fixing our attention especially upon the origin of the disease—that is, upon the original primary focus from which the process starts, and, consequently, the desirableness of following out those indications of treatment which have been particularly referred to this evening by Mr. Spencer Wells—the immense importance of watching against those circumstances which surround the infecting surfaces at the time which must necessarily correspond to the beginning of the process. Then, further, as regards the prosecution of pathological research, which seems to me very distinctly to point to what way we ought to work for the purpose of advancing our knowledge of pyæmia, what we have to do particularly is, not to study the ultimate results of the disease in post-mortem examinations, but to study the starting-point in each case, and investigate it, in the confidence that by this method only we can arrive at results corresponding to those which have been already arrived at in experimental pathology; to follow out, in fact, the same course that has already been followed in experiment, and to look for the primary indications of the channels of absorption. The examination of the lymphatic system in connexion with the foci of infection is a question of great importance in human pathology, as it is in the pathology of the experimental studies which have been already pursued. It is not the case that no studies of this kind have been undertaken, but they are very few. I may refer to one interesting set of cases lately published by Dr. Heiberg, of Christiania, with reference to puerperal pyæmia, in which this sort of careful anatomical investigation of the tissues immediately connected with the source of infection has naturally led to very interesting results. In surgical pathology there is no doubt that it might also be pursued, as it often happens that amputated limbs give us very excellent opportunities of obtaining the tissues about parts which are the seats of origin or centres of location of pyæmic infection, and studying such tissues in a perfectly fresh state.

DR. FAYRER.—I came here this evening with no intention of speaking, but as I have had unfortunately a very large experience in the disease which has been discussed here, I cannot sit still after the invitation that has been given me to say something. For the last thirteen years of my life I have had to do with one of the largest hospitals in India, where, unfortunately, pyæmia was only too rife in all its worst and most concentrated forms. I feel much diffidence and great reluctance to say anything in the presence of men whose names I have hitherto known, with whom now I have had opportunity of making personal acquaintance, and from very many of whom I have learned what little I may know excepting in so far as it may have been supple-

mented by practical experience and observation. I have been somewhat surprised to hear pyæmia spoken of as a surgical disease. Perhaps it may be that, being a military surgeon, and therefore also a physician, I am not accustomed to regard disease from the two points of surgery and medicine apart. Be that as it may, I regard pyæmia not one whit more surgical than it is medical or obstetrical. I believe you see the worst forms of it in all; I am perfectly certain that I have. I have not had the advantage of hearing the former discussions—I wish I had done so,—and I confess I have not read as I ought to have done what has been written on the subject; therefore I am hardly in a position to speak as to what may have been said here. I believe one point in the discussion has been whether pyæmia is simply a question of hospitalism, or whether it occurs in private practice. In my experience it occurs largely in both, and distinctly. I have always looked upon it as essentially a hospital disease, a result of overcrowding, of too little superficial and too little cubic space, from the massing of sick men together. How it is produced in that way I have never been quite able to know, but when I was at the height of my conviction upon that point, when for two or three or four years I had had the worst forms of pyæmia under my care that could possibly present themselves — something that made all surgical operations so hopeless that one dreaded to perform, especially if they involved a bone—I then also, under the most favourable circumstances, in isolated rooms at the top of well-ventilated houses, with every precaution that cleanliness and sanitation could devise, had some of the most aggravated expressions of the disease pyæmia. For example, a man operated upon for stone died after a few days (although for the first three or four days he had been doing perfectly well) with his knee-joint full of pus and his pericardium full of pus. Another man died in the same way after operation for stone, in an extreme state of jaundice. You may say that is not pyæmia; at all events it is blood-poisoning. I confess the term pyæmia is not one that to my mind commends itself. I am not aware that pus is taken into the blood, and if it be not, why should it be called pyæmia? Septicæmia is a better term, or even hyperæmia. Toxæmia, to my mind, is better than any, because it is an undefined term which expresses the presence of a poison. Where it comes from precisely I know not, but I believe it takes place in the presence of suppuration, not of healthy suppuration, not of the ordinary suppuration that takes place in a common wound or abscess, but under certain conditions, most frequently when men are crowded together in hospitals, but occasionally under other circumstances. It is quite unnecessary to detail the symptoms of the disease; Dr. Gordon has given them to you, and I have no doubt you know them all much better than I can describe them, but perhaps I might have said something about them as I have seen them. I believe they were in the most aggravated form, with rigors, profuse sweatings, and very rapid death. I should like to say something, because it is a subject that has always interested me, about the pathology, the structural changes that take place. I have heard constantly about diffused abscesses, and I have seen them frequently. I have seen a man take a long walk, become much fatigued, and very nearly die afterwards from diffused abscesses in the areolar tissue underneath the muscles. That I have no doubt is a sort of pyæmia. I have seen that take place in people who lived in crowded neighbourhoods, ill-fed, and living under unfavourable conditions. That is, I think, idiopathic pyæmia, but it does not follow wounds, and does especially follow section of bones where the medulla becomes involved in that condition, first described by a French surgeon, M. Jules Roux, as osteo-myelite, or osteo-myelitis. That, I think, has produced the worst form. Examination after death did not reveal collections of matter, but it revealed local gangrenes and local deaths in large numbers throughout the lungs, frequently in the liver, and not infrequently in the spleen. That they were abscesses I do not deny, but that they were abscesses in the first instance I do deny. They were deaths. Just as a bit of areolar tissue dies under the skin, and forms a core of a boil, around which suppuration takes place afterwards to get rid of it, so a portion of a lung died, or of the liver or spleen, the kidneys, or any of the viscera, and if the person lived long enough, if he did not succumb to the poison, suppuration took place around about these portions of dead tissue, and you found a collection of puriform or sometimes of purulent matter as the result; but very frequently death took place before any suppuration could possibly have occurred. That is one frequent result of pyæmia as I have observed it in Calcutta and in some other parts of India. But there is another and a peculiar condition of the blood—that poisoning of the blood, whatever it may be caused by, whether it be due to that indefinite thing, malaria, or whether it be due to some defective power on the part of the liver or spleen in sanguinification—which leaves the blood in a peculiar hypernotic condition, which makes it coagulate and form fibrinous clots when it ought not to do so. That in my experience has been one of the worst and most fatal results of the disease, which, for want of a better term, I have been accustomed to term "pyæmia" (but I have never been satisfied with the term),—clots forming in the right side of the heart or in the pulmonary artery, so causing death. These are conditions which we have been accustomed to look upon as results of what is called pyæmia—abscesses frequently under the muscles, in the liver, and elsewhere. Then, as to the curability of it. I think it would be hard to say it is always fatal; but it very frequently is. I believe if it goes on to structural changes, such as I have described, it always is. I am speaking of the disease as I observed it. I am inclined to think it is totally different in this country, because it is impossible that such things as I have seen could escape the observation of those who have studied the disease here. I only wish that there was someone here who had seen as much as I have of it in India, and who could speak upon the subject in the light in which I regard it. I think it may not be incurable at times. I remember one notable case in which a young man, who had all the worst symptoms of pyæmic disease with osteo-myelitis of the femur, recovered after amputation at the hip-joint, removing the whole of the affected bone, and doing it just in time before the structural changes took place, which must inevitably have resulted in his death. As to the milder cases, I should think that they frequently recover. It is much too late to speak about prophylactic measures; but I believe they are all comprised in these: in clean linen, sufficient space, proper nutrition, and good air.

Dr. MOXON.—I scarcely like, at this late hour, to occupy the time of the meeting, but what I have to say fortunately will compress itself into a very few words. This discussion has a similarity with one that has been carried on in a sister Society (the Pathological), and that similarity is to my mind instructive. In each case the inquiry is, as regards certain local changes, are they constitutional or not? And it is curious to observe that the modes in which the local is thought to become constitutional are very similar in the two cases. Indeed the question how a sore shall develop pyæmia is so like the question how a tumour shall develop a general carcinosis, that, I take it, it would be extremely difficult for anyone to define the difference. So that much of what is said in the sister Society will no doubt apply to the question before us, especially when viewed in the light to which I wish shortly to allude—that is, in reference to the probable existence of a general constitutional condition, which, I think, has not received quite so much attention from recent pathological writers as might have been devoted to it. I have myself seen a good deal of pyæmia, having inspected nearly all the cases that have occurred at Guy's Hospital during eight years. I heard something of the circumstances in each of these cases. Of course many of them turned out to be of little interest, being so common and ordinary; but a few had special features, and it was to bring forward the special features of two of the cases that I rose. The cases were not unprecedented. You, Sir, have mentioned two that were similar, and which occurred in your private practice; they were cases of pyæmia arising from gonorrhœa. I saw two of these cases myself, and I recollect further that I was made acquainted with another by Dr. Habershon, on the occasion of one of the inspections, when he mentioned to me that he had several years before seen a case of fatal pyæmia arising from gonorrhœa. I have also twice examined cases of idiopathic pyæmia: I do not mean of the kind alluded to by Dr. Sanderson—that is, pyæmia arising from acute periostitis; but cases where the person bursts quickly into a pyæmic fever, so that throughout the viscera, and in the joints, pus rapidly developed in immense

quantities, no local sores being discovered to explain the pyæmia. I searched for these carefully. Thus in one case, thinking that a carious tooth might have done it, I pulled out all the man's teeth to examine the maxillary antra, and I had previously looked through every other corner of the body. Those were certainly cases of idiopathic pyæmia. What else could they be? Now one cannot suppose but that some very grave constitutional cause must have brought about such a result as this, both in the gonorrhœal and the idiopathic cases. What should make one person with gonorrhœa get pyæmia when so many thousands escape? There must be something very special in the constitution of the person, and that something as a constitutional factor must surely be very important, when the local factor in the idiopathic cases is absent, and in the cases of gonorrhœa is so trifling. I examined the urethra in both cases of gonorrhœa, and there was no breach of the mucous membrane. Such cases appear to me to be parallel with scarlatinal rheumatism. Certainly one observes in young children who have not any ulcers in the throat, but only scarlatinous inflammation there, a certain amount of rheumatic symptoms, not always running on to pus, but often doing so. All these forms of the disease must be thought of before we can regard pyæmia from so narrow a point of view as would see in it only a local condition spreading along very defined lines, and extending until it secondarily affects the constitution, as some would teach. I think, when we look upon these cases—I speak especially of scarlatinous rheumatism—we must say that the constitutional side of pyæmia is of very great importance. And while I am speaking of scarlatinous rheumatism, I should like to remind the Society of certain very important statistics which Dr. Braxton Hicks has published. I believe he states that after investigating carefully the causes in a great number of cases of puerperal fever, he found a large proportion, about 65 per cent., were clearly traceable to a direct exposure to measles, diphtheria, or scarlatina. I think that statement is of the utmost importance, and, coming from Dr. Braxton Hicks, it cannot be doubted. I believe I am right in saying that this kind of investigation is rare. If such investigations were more general—if, as Mr. Spencer Wells has suggested, all the circumstances that could throw light upon the general conditions of the system in these terrible cases were inquired into,—if every contagion could be hunted up, we should get light on the subject that would be more fruitful in our clinical success than are those mere pathological investigations which are now so fashionable. I confess I was sick of cutting up the veins and slitting up the lymphatics; and when I came upon the accounts published by Dr. Hicks, it seemed to me that a new light was opened upon pyæmia, and I am still in hopes that the work initiated in this Society will be fruitful in teaching us something that will help us forward in this truly very promising direction.

Dr. GLOVER.—Would Dr. Moxon tell us what he saw in the veins and lymphatics?

Dr. MOXON.—It is a long story.

Dr. GLOVER.—If he has seen every case in Guy's Hospital for the last eight years, and made a careful examination of the lymphatics and veins, I am sure in a few words he could tell us whether he saw anything to explain the facts that have been referred to.

Dr. MOXON.—I may mention that in one case I should certainly call a case of pyæmia from the general conditions, there was a great wound about the hip which had led to swelling of the leg. On examination I found that the swelling arose from the inflammation about the wound having entered into the crural vein. The vein was full of clot, and the clot, when examined by the microscope, was pus. That is a point that I think has not been sufficiently asserted. It has been asserted, following Prof. Goodsir's observations, that clots in the veins are not pus. That is quite contrary to my experience. I quite admit that Goodsir was right in describing a process of disintegration of clot, but I am sure it is an entire mistake to say that clots that are found in the neighbourhood of suppuration, clots within the veins, are not pus. I say that in nearly all cases of thrombosis of the cerebral sinuses the clot is pus. It certainly was so in the case to which I referred. The clot ran right up the vein and pointed its nose into the right auricle. In the pulmonary artery of the upper lobe of the left lung there was a large piece of clot which, under the microscope, was identical with pus. The portion of lung which corresponded with that artery was broken down into a mass which would be called abscess. There were no other abscesses in the lung. I give that as a typical example of secondary suppuration, the real conveyance of the disease from point to point.

The PRESIDENT.—The time has now come when this discussion must close, and I will occupy the very few minutes that remain by speaking only on one or two points. First, it is quite clear that the discussion has brought out before us the great divergence of opinion existing among surgeons as to what is and what is not pyæmia. I find myself directly opposed to one of our first authorities and teachers in London, Mr. Erichsen; and also to another great provincial surgeon, Mr. Cadge. These gentlemen would limit pyæmia—at any rate, true, acute pyæmia—to abscesses which might be called visceral and multiple; so that any case that had no multiple abscesses and no visceral abscesses was not, for them, pyæmia. Now that is a case I cannot admit. I admit that pyæmia has external abscesses, and that they are not altogether confined to the internal organs. I admit, whenever I find pus in the joints, in the serous membranes, in the peritoneum or the pleura or the pericardium, that they are all cases of pyæmia. I do not want, for a case of pyæmia, multiple abscesses at all. If I have one abscess, I consider that a pyæmic abscess under certain circumstances. Mr. Erichsen pointedly asked me whether I considered the case of a slight wound or chafing on the heel, combined, as it had been, with a large deep-seated abscess in the thigh, to be a true case of pyæmia. I have no hesitation in saying that I do consider that a true case of pyæmia, and I cannot conceive why it should not be so regarded. As a matter of course, there are certain restrictions. Supposing it can be clearly proved by the surgeon (and one must take the surgeon for what he is worth) that there is no inflammation whatever of the absorbents spreading over the wound; that there is no inflammation spreading along the cellular tissue of the leg. If it had spread along the cellular tissue of the leg, it must have spread under the subcutaneous cellular tissue of the thigh, and there was nothing of that kind present. There was merely this small wound at the heel, and this large deep-seated abscess in the thigh. Therefore I must say—though I may be a teacher—that I teach differently from Mr. Erichsen. Then the discussion has brought out another point very markedly—that is, that pyæmia exists in private practice. And here I will refer to Mr. Spencer Wells's question. I had no intention, in bringing these cases before you, of instituting any comparison whatever between hospital and private practice. The title of my paper was "Cases of Pyæmia in Private Practice." I did not mention anything about hospitals; neither had I any intention of doing so. I merely wanted to show that I had seen a certain number of cases in the years in which I had been in private practice. I therefore cannot answer Mr. Spencer Wells's question, and give him the relative number of cases that I have seen in hospitals, and in private practice. Having settled that there are a number of cases occurring in private practice, it is clearly proved, as a matter of course, that all these cases are not due to what is generally called "hospitalism." And here I would say that "hospitalism" in this discussion has been used under two very different significations. It was first used by Sir James Simpson as meaning the hospital itself, and nothing else; and the deduction that he drew was that we were to pull down our hospitals and build new ones; that we were to divide the wards into smaller wards; that the larger the ward the more infectious it was; and that the older the hospital the more infectious it was. With such deductions as those, supposing them to be true, the word "hospitalism" is a proper word; but now I hear "hospitalism" described as being simply the overcrowding and aggregation of a certain number of bad cases in a ward. This has nothing to do with the hospital; it has to do with the surgeon, with the person who has charge of the hospital. It is simply a case of mal-administration. I confess I am one of those who, like Sir James Paget, regret very much the use of the word "hospitalism" in this new sense. I do not know that there is any word that can do more harm. When Sir James Simpson first brought out his work, wherever I went in my own neighbourhood I found people declining to become governors of hospitals. "What is the use of it?" they said. "See what Sir James Simpson says. You are to pull down your

hospitals and build new ones. You are not even to have those; you must have tents and huts." That was constantly said in the West-end of the town. It was a subject of conversation wherever I went. I have no doubt that the result was that our hospitals lost a great many subscribers. I therefore look upon the present use of the word with some little degree of fear, for we must bear in mind that these discussions are reported in the medical journals, which go into the clubs and are read there. Patients will now say (and very naturally), "What is the use of my going into a hospital? I had better die of my own disease than of one that I shall be sure to catch if I go there." Then there is another point that has been brought out very prominently in this discussion in reference to what has fallen from some gentlemen with regard to contagion. It has been said that hospital surgeons can carry this disease about with them, and convey it to their patients. Now, I think if the cases I have brought before you had been very carefully attended to, this question could not possibly have arisen. There were twenty-one cases under my care, and of those there were twelve that I literally had nothing whatever to do with, but was simply called in on consultation. Some of them were 200 or 250 miles away from London. All that I had to do was to say, "This is not a case of typhoid fever, but a case of pyæmia." And so it turned out. There were two cases of pyæmia with ulcer of the tonsils, and in both these cases pyæmic symptoms had set in before I saw the patients. There were three cases of pyæmia with fever. It is said that they were not cases of pyæmia. All I can say is that I looked upon them as such simply because there was ulceration of the bowel. If it is known that ulceration of the skin can lead to pyæmic symptoms, why may not ulceration of the bowel do the same thing? It is true that there is a form of fever called by Sir William Jenner pyogenetic, but in that case abscesses come on at a much later period than in the cases that I have brought under your notice. With regard to the case of injury to the foot, I was called to it fourteen or fifteen miles out of town, and the symptoms of pyæmia were there; the abscesses were there when I arrived. In one case I went 250 miles to see a young lady with inflammation of the lateral sinus spreading down the internal jugular vein. I told the surgeon that it was not a case of fever, but of pyæmia, and that in all probability in a week or ten days we should have abscesses. In a week or ten days the abscesses came, and I had to open them. Then there were three cases of gonorrhœa and another case of ulceration and abscess. All those cases I was called to see after the symptoms of pyæmia had set in, so that I have not on my conscience the crime of having taken pyæmia to my patients. Mr. Erichsen states that he has never seen pyæmia in private practice.

Mr. ERICHSEN.—No, no; after operations in private practice.

The PRESIDENT.—Mr. Erichsen has had a great number of cases of pyæmia in hospital practice. As a matter of course, they were contagious; and if the disease could be carried from one to another, why should he not have carried it about as well as myself? Mr. Cadge said that he was afraid of operating in a hospital because pyæmia was so rife, though he was confident enough with regard to operations in private practice. Why should not Mr. Cadge carry about pyæmia as well as myself? I think one of the very best proofs we have had was that given by the first speaker, Mr. Jonathan Hutchinson, who told us that he had a number of ewes that had died from pyæmia. Now, surely nobody could accuse Mr. Hutchinson of having given these ewes pyæmia, yet they all died of it. They were examined; they had multiple abscesses; and there was no doubt whatever of their having died from pyæmia.

The discussion was brought to a close with a vote of thanks to the President, on the motion of Mr. Callender, seconded by Mr. Erichsen.

THE deaths registered in London last week amounted in number to 1524, and included 52 from measles, 22 from scarlet fever, 66 from whooping-cough, 37 from different forms of fevers, and 8 from diarrhœa. The deaths referred to diseases of the respiratory organs and phthisis further declined to 584. To different forms of violence were due 49 deaths.

THE COLLEGE OF SURGEONS.

AT the Council meeting on the 12th inst. the following motion was proposed by Mr. Hancock, and carried:—

"That the Council, whilst expressing their great satisfaction on the formation by the Universities of Oxford and Cambridge of a Schools Examination Board, are of opinion that it is desirable that the examination be extended to other schools than to 'those having regularly constituted governing bodies,' or to 'those preparing a fair proportion of their boys for the universities.'"

Mr. Hancock seems to have taken the preliminary education of youth under his especial care, but, judged by his Hunterian Oration of last year, the standard he would propose is not a high one. There could be no objection, so far as we see, to the extension of the supervision of the Universities of Oxford and Cambridge to all private schools, if it were practicable; but this appears to us to be more the business of a School Board than of the Council of the College of Surgeons.

The following resolution, proposed by Mr. Birkett in his official capacity of chairman of the General Purposes Committee, was also carried:—

"That the salary of the secretary be increased; and that it be referred to a committee to advise the Council as to the amount of such increase, and for the preparation of a formula for the necessary alteration in clause 11, sec. xxvii., of the standing rules."

The duties of the secretary have been so much increased by the labours of the Conjoint Committee of Reference that it is quite right that they should be recognised in some way. It must be borne in mind, however, that this extra work will not last for ever, and that for the ordinary duties of the College the secretarial staff is sufficiently large, consisting of a secretary, assistant-secretary, and two clerks. In last year's Calendar the College department, as distinct from the Museum and Library, absorbed nearly £2500, of which £1372 went in salaries and wages; but we believe that the secretary's salary is by no means too large for the labour and responsibility of the office.

ON THE SAFETY OF TORSION IN AMPUTATION.

To the Editor of THE LANCET.

SIR,—The following case, brief notes of which I now forward, I think may be read with interest, supporting as it does the practice of torsion, and telling against the use of the carbolised catgut ligature.

On Friday, March 6th, I was called upon to remove a mutilated forearm from a man aged twenty. I amputated below the elbow-joint, and arrested bleeding by twisting all the arteries with the exception of the interosseous, to which I applied a ligature of carbolised catgut. I could not twist this artery on account of its intimate connexion with the interosseous membrane. At the end of six days hæmorrhage took place from the stump, which was stopped by raising the limb; but as it recurred towards night the stump was opened. It was then made out that the bleeding had taken place from the interosseous artery. The vessel was again tied by my dresser, and the man has gone on well since.

This is the only case of secondary hæmorrhage from a stump that has occurred to me since I began the practice of torsion of arteries in January, 1868; and it is interesting to know that in it the bleeding came from an artery that had been ligatured, and that the ligature was of carbolised catgut.

We have now had at Guy's Hospital upwards of 200 cases of amputation of the thigh, leg, arm, and forearm, in which all the arteries have been twisted (110 of these having been of the femoral artery), and no case of secondary hæmorrhage.

These facts are worth recording, and I send them to you with pleasure.

Yours faithfully,

Upper Brook-street. THOMAS BRYANT.

THE LANCET.

LONDON: SATURDAY, MARCH 21, 1874.

IN another part of this journal we publish the full report of the fourth and last evening's discussion at the Clinical Society on the subject of Pyæmia. With the different views which have been advanced by the various speakers concerning the nature and origin of this disease our readers have already been fully acquainted by the careful and accurate reports which we have furnished from time to time. But considering the difficulty of the subject, and the mystery in which it has been involved by the conflicting but definite statements of some of the most eminent members and teachers in the profession, it may not be unprofitable for us to take a general survey of what has been so recently said and down, and, as far as possible, impartially to sum up and estimate the evidence which has been given for and against every hypothesis which has been put forth concerning the essential nature of this disease. This week, however, we shall confine ourselves to a general statement of the questions in dispute, reserving for a future occasion the more detailed consideration of the pathology of the disease, the nature of the *materies morbi*, its mode of origin, and the character of the symptoms to which it gives rise.

It will be remembered that Mr. ERICHSEN, in his lectures on "Hospitalism" which have recently appeared in our columns, alleged that the large mortality which follows operations performed in hospitals is due to the influence of certain conditions, which, if not actually confined to hospitals, are so prevalent therein, and are so rare in private practice, that they may, practically, be regarded as intimately, if not necessarily, associated with hospitals, and may be designated by the term "hospitalism." Chief among the causes of the excessive hospital mortality in surgical cases is pyæmia, which Mr. ERICHSEN asserted has for years past proved fatal in an average of about 36 per cent. of all the deaths after amputations performed in the London hospitals, the proportion being lower in some institutions and higher in others. This gentleman, moreover, alleged that pyæmia is so rare in private practice that he himself had never met with a genuine case after operation, although he acknowledged that he had heard of such. So far everything appeared satisfactory and clearly proven by accurate statistics and extended experience; but soon a counter-blast was blown by the president of the Clinical Society, Mr. PRESCOTT HEWETT, who, in his address to the members of the Society, brought forward the details of twenty-one cases of pyæmia which had come under his own observation in private practice, and two other well-authenticated cases, of the particulars of which he had been informed; thus making a total of twenty-three cases, arising, it was affirmed, under the most favourable hygienic conditions, and in members of the higher class of society. This, therefore, was the point of divergence from which all subsequent discussion commenced; surgeons and pathologists of highest rank and standing ranging themselves on one side or the other, some alleging that pyæmia may practically be regarded as essentially a hospital disease—that is, that it commonly occurs under hospital conditions wherever existing; while others maintained with equal ardour the opinion that it is as frequent in private practice as it is in hospitals; and a few insinuated that it is even more frequent in private practice than it is in well-managed hospitals. To ingenuous and unsophisticated persons not fully acquainted with all the technicalities of this complicated question an appeal to facts may appear a very simple and definite mode of deciding whether pyæmia should or should not be regarded as one of the effects of the conditions which are known by the name of "hospitalism." Unfortunately, however, such appeals have hitherto resulted only in confusion doubly confounded, and have tended to involve rather than to elucidate the question at issue.

It must be confessed, therefore, that it is by no means an easy task to reconcile the contradictory statements of the two classes of disputants. The questions whether pyæmia is or is not a hospital disease, and whether it does or does not occur in private practice, are not so simple as they at first appear. That it may occur and has occurred in private practice is indisputable, but it is contested that in such cases the disease either did not originate therein, or was at least dependent on the operation of causes identical with those which so commonly prevail in hospitals. It is a mere waste of words to attempt to argue away the large influence that pyæmia exerts on the mortality of hospitals; or, on the other hand, to deny the frequent and intimate relationship which exists between hospital conditions and the occurrence of pyæmia. The facts stare us in the face, modify them or contort them as we may. It is doubtless true, as Sir JAMES PAGET has said, that in a well-managed hospital pyæmia will not be more frequent than in private houses; but this is really the giving up of the whole question in dispute. Those who maintain that pyæmia is a hospital disease, and that it is only found where hospital conditions are known to operate, do so, we presume, only on the evidence afforded by experience of the majority of large surgical hospitals as they really exist. It is evident that if all hospitals were actually as perfect as theoretically they may be rendered, and if every conceivable fault were removed and the management and administration made incapable of improvement, then the influences which now prevail would no longer exist, or would at least be reduced to a minimum. But the contingencies of life are so capricious, and our physical and moral resources so limited, that it is doubtful whether we shall ever attain to that degree of perfection which will convert our hospitals from lazar-houses into the purest sanatoria and convalescent homes. Mr. CADGE, for instance, in his able and practical remarks at the Clinical Society, stated that at the Norwich Hospital pyæmia had been on the increase for many years past, notwithstanding all that had been done to improve the system of drainage and ventilation. The explanation of this increase he finds in the overcrowding which has necessarily occurred during the last few years, from the increase of the population (the size of the hospital remaining the same), and from the larger number of serious wounds and injuries which have been thrown

on to the hospital by the more extended use of mechanical contrivances to lighten manual labour, and particularly by the more general adoption of agricultural machinery. The surgeons at the hospital were, however, unable to battle with the evil, except by restricting the admission of patients or by enlarging the hospital, the former of which was hardly practicable, and the latter for various reasons was not till recently possible. We dare say that hundreds of hospitals in this country could afford evidence similar to that at Norwich. The fact is that, if we speak only of well-managed hospitals in the sense we apprehend Sir JAMES PAGET to mean, we should probably have to exclude the majority of our hospitals, the exceptions being for the most part found in institutions liberally founded in districts the population and traffic of which have not materially increased or altered, so that their resources are never strained beyond their available limit, or in some of the new hospitals built on the most approved plans.

If we now turn to Mr. PRESCOTT HEWETT'S experience, and for the moment accept it as representative, it will be impossible to avoid the conclusion that pyæmia is really more frequent and is more liable to occur in private than in hospital practice. For Mr. HEWETT laid great stress on the superior hygienic conditions under which his patients were placed, the robustness of the previous health in most of the cases, and the high social position of all. Yet, notwithstanding the combination of all these circumstances, which had usually been regarded as inhibitory of so-called purulent infection and of blood-poisoning, the patients were seized with pyæmia, to which many succumbed. Whereas, in hospitals, we almost invariably find that the outbreak of pyæmia, or even the occurrence of an individual case, is preceded by the overcrowding of patients, or the aggregation of a large number of wounds in a small space, or by the operation of a series of faulty hygienic conditions. How is this difference to be explained? Conflicting as this evidence may appear, the dispute may be regarded as a verbal one, arising from the application of the same term to different if not dissimilar conditions, rather than from any inconsistency in facts. Trustworthy clinical observers have not, indeed, hesitated to challenge the accuracy of the diagnosis, or, more correctly perhaps, the designation of several of Mr. HEWETT'S cases. Mr. ERICHSEN and Mr. CADGE, for example, have expressed serious doubts whether many of the cases which have been recorded as genuine pyæmia can be so regarded. It is evident, therefore, before we can make any real progress or arrive at a solution of this difficulty, we must clearly define what is meant by the term "pyæmia," as ordinarily applied. This we shall endeavour to do next week.

BY the end of the present month the sands of the present Director-General's official hour-glass will have run out. Director-General succeeds Director-General. Hopes rise and vanish, and praise alternates with blame. The effect of a Director's-General's influence is very different from that ascribed by GOLDSMITH to his village preacher: truth does not prevail with double sway when pronounced by official lips; and the scoffer is not turned into the worshipper by a contemplation of the best personal qualities. We often ask ourselves what it is that can make a man desirous of directing a public service like that of the Army Medical Department. His official career is beset with difficulties. His responsibilities are great, and his liberty of action is very small. He has to steer his craft through official shallows and between rocks of which no one knows but himself; and he has occasionally, when stranded on a hostile shore, to sacrifice a valuable part of his cargo in order to avert imminent shipwreck. Be his skill and success what they may, he will not escape unfriendly criticism; and he is lucky indeed if he do not reap obloquy as his reward. He finds himself compelled by his position to maintain silence as to his motives, and to refrain from divulging the secrets of his office and the obstacles to his success. The present Director-General is stated to have observed, in reply to some adverse criticism, that "he could only desire to have all he had written and striven to accomplish for his department placed upon one of LANDSEER'S lions for the world to read." The character of many a conscientious and honourable-minded official has been sacrificed to that low-toned political morality which ascribes the credit of all that is creditable to the political party in power, while it affixes to the head of a department the discredit of every failure. Where is the Director-General who has been able to count on the sympathy and support of his department? We have no doubt that the present Director-General will not escape the fate of most officials on their retirement. He will have to cast up the sum-total of frustrated designs and unrecognised because unknown labours. All the world now knows, for they have been told by the late War Minister himself, that Sir GALBRAITH LOGAN had previously brought to his notice all the different matters out of which so much dissatisfaction has arisen; but it will probably never be known how far the Director-General's original scheme would have averted all this dissatisfaction. The changes which have taken place have, at any rate, been in the direction he had indicated. In a private and social capacity the Director-General will bear away with him the esteem and affection of all who enjoyed his friendship; and, in looking back on his official career, he will be able to recall that no medical officer could say that he approached his chief with fear, or left him with a feeling of injured or diminished self-respect. Let his rank have been what it might, he was accorded the treatment of a gentleman by a gentleman. Many a man who would have remained for years an assistant-surgeon will have to thank him for his promotion; and if the suicidal policy of the deputation to Mr. CARDWELL had not been adopted in regard to the restoration of the half-crown a day, the number of such promotions would have been much increased, and the War Minister's unwritten pledge of promotion after fifteen years' service would have amounted to a guarantee which no succeeding Government could have overlooked. Last of all, the Director-General's tenure of office has been an epoch in the department. The initial step, for good or evil, has been taken for unifying the department. If this change has been brought about suddenly, imperfectly, or clumsily, the chief of a department is not a power in the State, and able to give effect to the principles on which his policy is founded. Amid the radical changes to which the army was subjected, it was not to be expected that the medical service

would remain untouched; and the best attainable is all that any official can expect to secure. It is, however, in the shallow waters and amid the surf that danger and discomfort arise; but safety and security are realised once the open sea is reached; and we hope that the next chief may have a better chance in the future of steering his craft in "the open" with more credit and efficiency to the service than were possible in the troubled waters of the past.

SINCE THE LANCET of Feb. 7th appeared, we have, in accordance with an intention expressed therein, been at some pains to arrive at an accurate knowledge as to the actual and comparative number of metropolitan hospitals the managing committees of which possess a medical element. In pursuing this inquiry we eliminated Guy's, St. Bartholomew's, and St. Thomas's from the list, because, as endowed hospitals, their system of governance is different from that of all other London hospitals. With these exceptions, all hospitals are comprised in the following list that have as many as, or more than, 50 beds. They are, St. Mary's, Middlesex, Charing-cross, University College, King's College, London, Westminster, St. George's, Evelina, Epileptic, Fever (Liverpool-road), German, Great Northern, Lock, Ophthalmic (Moorfields), Small-pox (Highgate), West London, Brompton (Consumption), Royal Free, Seamen's, Victoria-park, and Children's (Ormond-street) Hospitals. These form a total of twenty-two institutions. The medical element of management may be thus epitomised:—In fifteen hospitals the numbers of the medical and surgical staff are directly and effectively represented in the committee, and vote accordingly. In three cases—viz., at University College, the Ophthalmic, and the Lock—the medical element appears to be only indirectly represented. In one case (the Seamen's) one medical subscriber was added to the committee about two months ago; but as this gentleman (a naval officer) has just been ordered to Jamaica, the Seamen's Hospital must still be classed with the remaining three institutions—namely, the London, the Royal Free, and the German Hospitals, on the managing committees of which no medical element exists. It will thus be seen that, in spite of much that has been declared to the contrary, medical representation in hospital management now exists in more than eighty per cent. of our large metropolitan institutions. The justice of this arrangement is, to our minds, incontrovertible. We have collected these statistics to aid in the settlement of a somewhat vexed question, and it now remains for the authorities at the London, German, Seamen's, and Lock Hospitals to show if the exceptional feature in their organisation has or has not specially advanced the usefulness and success of these institutions.

AT an inquest held at King's College Hospital a few days ago Mr. Bedford, the coroner, referring to a letter he had received from a medical officer to the hospital, claiming a fee for post-mortem examination of a person who had died in the wards, said the law was perfectly clear on the subject, and did not enable him to allow a fee in any case in which death had not occurred before admission into hospital. This statement of the coroner cannot be too widely made known amongst the medical officers of the metropolitan hospitals.

Medical Annotations.

"Ne quid nimis."

THE COUNCIL OF THE COLLEGE OF SURGEONS.

WE have received a report of a meeting of the Fellows of the College of Surgeons residing in Birmingham and its neighbourhood, at which it was resolved to seek a further extension of provincial representation in the Council of the College of Surgeons by promoting the election of Mr. Alfred Baker, of Birmingham, into the Council next July. Mr. Baker is a provincial surgeon of eminence, who has been brought prominently before his fellow-townsmen as Senior Surgeon of the General Hospital, and as President of the British Medical Association two years ago, and if he is prepared to make the sacrifices necessary for the due fulfilment of the duties of the office, we see no reason why he may not fairly aspire to a seat on the Council. Mr. Baker is, like Mr. Southam, a Fellow by election, and his seniority is, therefore, only of 1852. As a Member of 1837 he is, by seniority in the profession, fully entitled to a seat, unless, indeed, more eligible Fellows senior to him should be brought forward on the coming occasion.

We believe on good authority that one reason for the anxiety of the Birmingham Fellows to be represented on the Council is the idea that the provincial students do not always receive fair play because their schools are unrepresented in the Court of Examiners. Such an idea is, we believe, quite erroneous; but as it exists we may as well point out that, from the nature of the case, the more complete representation of the provincial schools in the Council can have no effect on the Court of Examiners, since provincial Fellows are unable (as was found in the case of Professor Humphry) to discharge the duties of examiners. But even if this were not so, the approaching completion of the Conjoint Examination scheme will separate altogether the councillors from the examiners, who will be appointed by the Committee of Reference. If, however, the Birmingham teachers think they have just cause of complaint of any examiners, let them depute one of their members, who is a Fellow of the College, to attend the examinations, and report upon the subject to the President if occasion for complaint should occur.

MR. GLADSTONE DE SENECTUTE.

WE have too much regard for Mr. Gladstone and for the public interest to acquiesce too readily in his appeal to be let off work soon on the score of "age." The feeling of age is very deceptive as a guide, especially after great exertion followed by a great reverse. Mr. Gladstone is in danger of confounding the mere sensations of fatigue and the mere "want of rest" which he so justly expresses with the feeling of "age." He is too young yet, in point of years at least, to speak as he does of "my age." Such language is not that of really old men, but of men that feel the contention within them of younger forces and older feelings. And in Mr. Gladstone's case we look hopefully for further triumphs of the youthful forces. Nothing is more striking or more pleasant to think of than the recuperative power of great men, of which not a few instances might be adduced from the case of living statesmen. And Mr. Gladstone, if he has a graver cast of mind than is perfectly favourable to longevity, has also many qualities which favour recuperation. Conspicuous among these is his ability to change his occupation. When the country can spare him for a time, which is the simple significance of the recent election, he can betake himself to "fresh woods" of Greek literature, to the study of Wedgewood-ware, or of old and new phases of religious thought, with occasional reversion to his favourite exercise of felling trees.

We repeat, men are not always judges of their own age. Sir Edwin Landseer, in 1842, discovered a long-lost picture which he had painted in 1818. He thought it a pity to alter it, and so to mingle his early style with that of his "old age." But after that allusion to his old age in 1842, Sir Edwin lived a generation to paint pictures which will be the delight of generations yet unborn. Compared with other potential men in Europe, and with other statesmen who have served England, Mr. Gladstone is not old. Before we assent to his superannuation, we shall have to be assured that rest has not made Mr. Gladstone fit for work again.

> The sweet vicissitudes of rest and toil
> Make labour easy, and renew the soil.

While he is getting the rest for which he has so greatly worked, other vicissitudes will be telling in his favour—those of public opinion. Mere years are no disqualification for a statesman; more even than in the case of a physician, they have their advantage. The real question is not, can Mr. Gladstone work more? but has he the power of resting? Mr. Disraeli has shown during the last to him dreary five years that he has this power, and now he comes forth refreshed and fit for new services. Periodical rest is the physiological condition of political life, as of other kinds of work. The way in which it comes about may not be agreeable to public men, but the result is highly beneficial both to them and to the public; and we shall be curious to see whether Mr. Gladstone numbers among his wonderful faculties that of taking rest.

THE ASHANTEE EXPEDITION.

MUCH of the intelligence received by us has been anticipated by telegrams and special correspondents' letters from the Gold Coast; still there are several items of interest remaining that appear to us worthy of being recorded. The outcome of the intelligence, as regards the sanitary condition of the troops, amounts to this—that the total sickness has been large, and the mortality insignificant. Perhaps the character of the sickness could not be better described than in the terms of Sir Garnet Wolseley, that a large majority of the cases were not more serious than a bad cold in the head would be in England. A medical officer of the *Victor Emanuel*, writing on the 22nd ult., speaks of 50 officers and 500 men having passed through their hands, with only two deaths among the former, and three among the latter. The cases of dysentery are among the worst and most troublesome met with. The wounded are spoken of as doing well; and, in connexion with this subject, it is very gratifying to know that Sir Garnet, after visiting the *Victor Emanuel*, was able to state that he never saw men better looked after or more comfortable; that the vessel was everything that a hospital ship should be, and without the least atom of hospital smell about her, although she had a number of wounded on board. *En passant* we may remark that this ship was to leave Cape Coast towards the end of last month, stopping at Sierra Leone, St. Vincent, and Gibraltar. All the slight cases and convalescents were to accompany their regiments, the severely wounded, dysenteric, and bad fever cases being relegated to this hospital ship. We observe it stated by the special correspondent of one of our daily contemporaries that a feeling of great disappointment was prevalent in the regiments at the chance of their being landed at Gibraltar instead of going home direct. As far as the doctors are concerned, we believe there is not now, nor has there ever been, any such intention. On the contrary, we have always understood that the medical service was desirous, on both moral and sanitary grounds, that the sick should be forwarded to this country, and that provision should be made at Gibraltar for such exceptional cases only as were manifestly unfit to continue the voyage or incur the risks of a more rigorous climate. As Sir Garnet Wolseley's military policy in somewhat precipitately quitting Coomassie, after having taken such infinite pains to get there, has been criticised, we may state what is the judgment of military doctors on this point. They hold that the General deserved as much if not more credit for leaving Coomassie when he did than for getting there when he did. Everything depended, in their opinion, on the exercise of his sagacity and sobriety of judgment in this respect. The rains set in earlier in the interior, and with the rains disease and vastly increased difficulties in the march back; moreover, the inactivity of the troops following their previous hard work and excitement, in a horribly insanitary place like Coomassie, might in all probability have led to the outbreak of some disease. Slightly to vary the application of a quotation to Sir Garnet, it may be said that nothing in the expedition became him so well as his manner of quitting it. By the latest accounts Sir Garnet Wolseley was in excellent health, and only awaiting the arrival of his successor, Mr. Berkeley, at Cape Coast to take his departure, hoping to reach this country before the end of the present month. The arrangements for the conveyance of batches of invalids from post to post, from the front to the base of operations, are allowed to have been excellently designed and well executed; and, take it all in all, the Ashantee war may be looked upon as a small affair from some points of view, but this nation has rarely executed a more difficult or unpromising undertaking, or one that has been better planned; and the proof of the excellence of the plan is to be found in the fact that Sir Garnet Wolseley has followed it out, spite of unforeseen difficulties, with the regularity and certainty of clockwork. We always hear of official blunders, but on this occasion, at any rate, our permanent officials have had it all their own way, and did not blunder.

METROPOLITAN HOSPITAL SATURDAY.

IT is to be hoped that the mutual civilities which are being exchanged by the Council of the Hospital Sunday Fund and the Committee of the Hospital Saturday Fund will be still further developed, until a complete unification of the machinery is accomplished. Perhaps the efficient organisation of a scheme for a Saturday collection would require a body larger than the present council, and one somewhat differently constituted; but the council has power to add to its numbers, and it is very desirable that the Saturday and the Sunday collections should coalesce, so as to form one fund; otherwise the administration of the two funds might clash, and the distribution be faulty in various ways. When the representatives of all forms of charity in the metropolis are drawing together, it will be unseemly, to say the least, if the friends of hospitals and dispensaries, designing the one object of supporting these institutions, contrive to separate and to act inharmoniously. Union is strength.

AID FOR THE SPANISH WOUNDED.

A LAUDABLE enterprise seems in danger of shipwreck from the intrusion of sectarian partiality. The civil war now raging in the north of Spain is attended with the severest privations, mainly due to the want of ambulances and medical resources. Neither Carlist nor Republican army is at all adequately provided with the necessaries of civilised warfare. After the engagement at Estella hundreds of the wounded were lying in the open fields—in mid-winter, be it remembered; so that many died from want, not of medical assistance merely, but of shelter and sustenance. The cabins of the country could contain only a small proportion

of the worst cases; and in mountain warfare, aggravated by winter weather, we may imagine the suffering and the mortality. To mitigate such horrors a Committee of Relief has been established in London; but though its appeal is in behalf of the wounded Spaniard, its ultramontane membership creates the suspicion that the funds it collects will go to the Carlists only. To obviate such an impression, the Committee has only to reinforce its numbers by a few Protestants of mark—say Lord Russell, Mr. Newdegate, Mr. Whalley, and Dr. Cumming—to reassure the public that their donations will not go exclusively to the Carlist, or, in other words, the Catholic cause, but will be impartially distributed among the wounded of Marshal Serrano as well as among those of General Saballs.

PRELIMINARY EXAMINATION AND THE CONJOINT SCHEME.

THE fourth report of the Committee of Reference, which we published last week, on the subject of Preliminary Examination, recommends an addition to the number of examinations recognised by the Council—viz., that of the junior local examinations conducted by the English universities. The committee anticipates that the number of candidates for whose preliminary examination they will be responsible will steadily dwindle until the necessity for a separate board shall cease. Meantime it is to be regretted that none of the examining bodies have gone fully and heartily into an investigation of the question of the rejection of such large numbers of students at the preliminary examination. Mr. Gay saw the importance of this subject, and got a committee appointed by the Council of the College of Surgeons to inquire into it. He showed that from 20 to 50 per cent. were rejected at the different preliminary examinations, that candidates were generally rejected in more than one subject, and that the subjects in which they mostly failed were Latin, Greek, Euclid, and Algebra. Unfortunately, the Council of the College changed its own mind, and retracted Mr. Gay's motion at a subsequent meeting. If the examining bodies allow this important question to drop, and their supervision of preliminary examinations should cease, they will suffer a great opportunity of throwing light on the efficiency of our educational methods to go unimproved.

CHARITY VOTING.

AT a recent election of a surgeon to the Cumberland Infirmary, the Dean of Carlisle, who presided, made some sensible remarks upon the folly of the present voting and proxy systems in charity elections. We have so often pointed out the actual hardships to which medical men are exposed in canvassing a large body of subscribers, that we need not refer to it again, except to express our gratification that a well-known leader in the charitable world like Dean Close should have spoken out boldly against the existing system of proxy votes and open elections. The very reverend gentleman said:—

"At this moment, I am told, we have seventy persons in the hospital, in its unfinished state; therefore it has become an institution of very great position in the country, and we ought to have the very best and most skilful officers that we can select. That makes it all the more important that the mode of election should be reconsidered. I should be glad to hear the opinion of the gentlemen present, whether they concur—not perhaps in the particular plan I have sketched—but in some plan which will prevent the election of officers being thrown over the entire body of governors, who now number something like 170 or 180. I am happy to say the number has been increased 45 by donations to the building fund; so that we have a very large body, widely distributed, and many of them far from the means of information and the centre of action, and it becomes the more essential to provide for the future some better plan than that of universal canvassing and universal voting by proxy has proved itself to be. For myself, I am dead against such a system of voting in charitable institutions. I utterly refuse to give my votes in all elections in London, because I regard it as quite a mischievous system, in which the merits of the case are little regarded, but everything is done because Lady Betty So-and-So and Lord Francis So-and-So choose to spend a deal of money on circulars and so forth in order to get their pet candidates in; and the proxy system approximates to that."

THE ROYAL MILITARY ACADEMY, WOOLWICH.

THE Report of the Board of Visitors to the Royal Military Academy, Woolwich, for 1873, remarks on the very satisfactory condition of the general health of the cadets. The medical officer, it appears, directed the attention of the visitors to the want of a sheltered communication between the sleeping-rooms and the baths in a portion of the building, and expressed an opinion that a few cases of illness were traceable to the exposure caused by the absence of this provision. The Board of Visitors, in concluding their report, call attention to a subject which they consider of much importance—viz., the overruling of the decision of the Medical Board appointed to examine the candidates for admission to the Academy. It appears to them very doubtful whether it can be prudent to permit of appeals from the decision of a trustworthy medical board, it being, in their opinion, far more important to insure the rejection of those of doubtful strength than to guard against the occasional rejection of one who might be stronger than the board supposed. If such an appeal be permitted, however, it should be to authorities of such professional eminence that other medical men would not hesitate to yield to their united opinions.

M. RICORD AND THE SULTAN.

WE mentioned, two years ago, when M. Ricord and M. Demarquay undertook a journey to Constantinople, that something more than the wish of seeing the Bosphorus induced these gentlemen to face the fatigues of such a journey. This opinion, shared by everyone, receives now confirmation by the fact that M. Ricord has been presented by the Sultan with the order of Commander of the Medjidié, and M. Demarquay that of Officer of the same order. This distinction must be the more flattering to the recipients as so many months have elapsed since their visit to the East. Their advice has probably been followed by excellent results—so excellent that the forgetfulness so common with patients has given place to an honourable recognition. It will be interesting to learn, for the guidance of our descendants, what was the figure of the immediate recognition of Ricord's services.

HOSPITAL OUT-PATIENT REFORM ASSOCIATION.

THE annual meeting of the Hospital Out-Patient Reform Association was held on Wednesday evening, in the rooms of the Medical Society, Chandos-street, Dr. Alfred Meadows in the chair. The Secretary, Mr. Nelson Hardy, reported that replies had been received from 30 hospitals out of 48 in answer to recommendations sent them by the committee on the subject of reforming the out-patient system. Of the 30 hospitals the authorities of 17 replied in general terms, while 13 sent specific answers to the recommendations of the committee. With regard to the first resolution—viz., "that each patient should be seen personally by one of the medical or surgical staff, and that no unqualified student be permitted to prescribe for out-patients," eleven said that the rule was already in force. As to the second recommendation, "that a lay officer be instructed to see

that the charity is not abused by persons coming who are able to pay," nine replied that the rule was already in force. With regard to the third recommendation, having reference to not giving medicine gratuitously, eight absolutely rejected the proposal, while five stated that medicine was paid for by the patients. Dr. Anstie moved the adoption of the report, which was unanimously carried. After some discussion, a resolution condemning the present system of out-patient relief was agreed to.

THE DENTAL HOSPITAL OF LONDON.

THE authorities of this hospital celebrated the opening of their new buildings in Leicester-square on the 12th inst. by a dinner at Willis's Rooms. Mr. Campbell De Morgan presided. Nearly 200 members of the Odontological Society and others were present, and the guests of the evening included the President of the College of Surgeons, Sir William Fergusson, Sir Ranald Martin, C.B., Sir Charles M'Grigor, Mr. Christopher Heath, the President of the Pharmaceutical Society, Mr. T. H. Hills, Dr. Steele, and many others. In acknowledging the toast of the evening, with which the name of Mr. Edwin Saunders was specially connected, the speaker reviewed concisely and completely the past history of the institution, detailed the difficulties that had arisen as to the removal, and how satisfactorily these difficulties had been overcome.

We have heard that the "Mayfair Minstrels" are organising a concert for the benefit of the hospital, which will be held in the large room on the upper floor, or in the museum. In thus indicating once and again the praiseworthy and continuous exertions of the promoters of this hospital, it is proper to remind those in whom its governance is vested that they have, with a new lease of life (and, as we hope, of success), taken also a new lease of responsibility. The College of Surgeons have granted to this branch of the profession special privileges, and it is the duty of the Odontological Society to guard these privileges jealously, to eliminate the so-called black sheep, if any exist, and to maintain and support the dignity of the College to which they are affiliated.

THE ATCHEEN EXPEDITION.

HAVING concluded our own war, we heartily desire to hear of a successful issue of the Dutch campaign in Atcheen. We have had our political battle over the Straits of Malacca in this country; but we can learn very little of the progress of the Dutch expedition in that part of the world. On the 24th of January the "Kraton" of the Sultan was taken without any loss, for the enemy had evacuated the fortress. Although this is a great step gained, there seems to be some chance of the war being prolonged over a period of one or two months, and if such be the case the losses by sickness may be great.

HOSPITAL SATURDAY IN BIRMINGHAM.

THE Hospital Saturday movement was for the second time on trial on Saturday, March 14th, and although the sum at present received (£3409) is far below what was gathered from the same source last year, there is much cause for thankfulness at the largeness of the sum collected, and for congratulation of the working classes on having voluntarily assumed so considerable a part of the responsibility of supporting those charities of which they are, in cases of accident or disease, so ready to avail themselves. Last year the movement derived strength from several causes which have not been in operation this time. Novelty gave its charm to the effort; Mr. Gamgee was its indefatigable pioneer and champion, and Mr. G. F. Muntz gave £500 to pay all expenses incident to it, so that the net proceeds reached £4700. More may be done on future occasions, but for what has been already done much praise is due.

THE DIRECTOR-GENERAL OF THE ARMY MEDICAL DEPARTMENT.

WE understand that the appointment of Sir William Muir, K.C.B., as the next Director-General of the Army Medical Service, on the retirement of Sir Galbraith Logan, K.C.B., has been definitely settled; and the service is to be greatly congratulated on the fact. Of his past services there is no need to speak; but Sir William has had the lion's share of the medical arrangements for the Ashantee campaign, so that in his appointment "the battle is to the strong."

THE THURSLEY POISONING CASE.

AT the adjourned inquest on the bodies of Thomas and Charlotte Coombes and their three children, held at the Half Moon Inn, Thursley, Dr. Stevenson, of Guy's Hospital, deposed that he had found arsenic in the viscera of the five bodies. The coroner having summed up, the jury returned a verdict of "Death from accidental poisoning," accompanied by an expression of opinion in favour of compelling manufacturers of sheep-dressing liquids to paint the word "Poison" on the tubs containing the composition. We hope this sad case will act as a warning to farmers throughout the kingdom.

DR. HARRIS BUTTERFIELD, medical officer of health for Bradford, in his report of the health of the town for the last quarter in the past year, comments on the excessive mortality of infants now observed in our large towns. This mortality he attributes in a great measure to the too early weaning of infants by mothers employed in factories, and to the ignorance of mothers on the subjects of feeding and clothing. In Bradford children within the first hour of their birth are often fed with bread slops, butter, sugar and gruel, and not unfrequently a child is dosed with castor oil before he is a day old. Among certain classes there exists a very low estimate of the value and importance of infant life, expressed by the too common saying that when a baby dies it is "better off." This feeling often leads to culpable neglect of the means of preservation.

THE tradesmen of the parish of Lambeth would appear to be an exceptionally honest set of people. Mr. John Muter, the public analyst of the district, in a report just issued, states that out of 138 articles of food and drink analysed by him during the past three months, 7 only were found to be adulterated. Sweets, including, we presume, "Conversation lozenges," were examined with the result, according to the chairman of the Lambeth Vestry, that "no trace of adulteration was found in them." Such news seems too good to be true. However, we congratulate the Lambeth tradesmen.

IN addition to the £1000 which we have already announced as Mrs. Neil Arnott's intention to present to Edinburgh University for the encouragement of Natural Philosophy, the same munificent lady has intimated her desire to give an equal sum to the sister universities of Glasgow and Aberdeen respectively for the same purpose.

A COTTAGE HOSPITAL erected to the memory of the late Marquis of Westminster at Shaftesbury, Dorsetshire, was opened a few days ago by the Bishop of Salisbury, who delivered an impressive address on the occasion. The building cost about £2000.

THE Sanitary Committee of the St. George's-in-the-East Vestry have made a novel and rather startling suggestion. They recommend that in carrying out the provisions of the Adulteration Act the names of tradesmen in the parish be distinguished by numbers, and a ballot taken as to those whose articles shall be first analysed. When this is done the analyst and his inspector will report on the number given them, the name of the tradesman being kept a secret until legal proceedings should be deemed necessary. The vestry unanimously adopted the suggestion.

CAPTAIN OMMANNEY, in a report on the sanitary state of Peshawur, states that the registration of births and deaths had much improved during the past year. Deaths are registered in two places; at the City Dispensary, where the reports of the relatives of the deceased are received, and at the city gates, where every dead body passing through for burial or incremation is inspected by the police. The two sets of entries are compared, and discrepancies detected. Violators of the registration bye-laws are prosecuted and fined.

AN inquest was held on the 13th inst. at St. Helen's, Lancashire, on the body of an infant who when a fortnight old had been vaccinated at the workhouse, and died a fortnight afterwards. The death appears to have been regarded, somehow or other, as the result of vaccination. The medical and other evidence, however, satisfied the jury that the supposition was groundless, and the following verdict was returned:—"Died from natural causes, accelerated by exposure to cold."

A CORRESPONDENT of the *Daily News* gives the following extract from a letter received from Zanzibar on the subject of Livingstone's remains:—"Latest news is that Murphy is bringing down Dr. Livingstone's body to the coast, which, after having been exposed to the sun for a month to dry, and then packed in a hollowed tree, was wrapped round with cloth, and the natives carrying it suppose it a bale of cloth, or kaniki."

WE have much pleasure in announcing that H.R.H. the Prince of Wales has graciously consented to preside at the festival of the Royal Medical Benevolent College on Wednesday, April 22nd, at Willis's Rooms. We hope that the profession will meet his Royal Highness in large numbers, and that the funds of a deserving institution will receive a very solid addition from this mark of royal favour.

THE Charity Organisation Society held its annual meeting on Wednesday last, under the presidency of Mr. W. E. Forster, M.P., who delivered an able address. Mr. Walter, M.P., Mr. Sclater-Booth, M.P., and the Rev. Harry Jones also spoke. We shall next week refer at greater length to this meeting.

THE Board of the University of Dublin have sent the Mansion House Committee a cheque for £100, for the relief of the sufferers by the famine in Bengal. The Governors of the Bank of Ireland have also given £200 for the same charitable object.

DR. CUNNINGHAM, Sanitary Commissioner for the Government of India, will make a communication at the Medico Chirurgical Society on Tuesday on the subject of cholera in India.

THE Astronomical chair in Trinity College, Dublin, is now vacant by the resignation of Dr. Brunow. The emoluments of the post are £610 per annum, with a free house and fifteen acres of land.

DUNDEE PRISON is so frightfully overcrowded that Dr. Miller, the medical officer, reports it—the female department especially—to be dangerous to the health of its inmates. Instead of the regulation 500 cubic feet of breathing space, only 143 were given in some cases. Dr. Miller suggests the desirability of hospital accommodation being provided within the prison.

THE *United Service Gazette* states that Deputy Inspector-General of Army Hospitals F. Reynolds, half-pay, has been appointed resident medical officer to the troops at Castlebar, with a salary of £100 per annum, under the new arrangement for providing professional attendance to the soldiers at small garrisons.

THE inhabitants of Notting-hill complain, and on just grounds, of the offensive discharge of refuse from the Gaslight and Coke Company's works into the public sewers. The vestry have now taken out a summons against the Company at the police-court. A protracted hearing of the case is expected.

THE Lambeth Guardians have decided, after some opposition, to build a new infirmary adjoining the new workhouse in Brook-street, Kennington. The decision was arrived at in preference to converting the old workhouse into an infirmary.

PROFESSOR HUXLEY has commenced a series of lectures "On the Phenomena of Life as Motion and Consciousness," in the theatre of the School of Mines, Jermyn-street. These lectures are designed for working men.

SCARLET FEVER and small-pox are reported from several parts of the county of Durham. At Consett the former disease is very prevalent, four deaths in one house being recorded.

DR. STOKES has been elected President of the Royal Irish Academy, in the place of the Rev. J. H. Jellett, F.T.C.D., whose term of office has expired.

SMALL-POX has disappeared from Stranraer, N.B., not, however, before giving the townsfolk such a lesson as to lead to the erection of a small-pox hospital.

THE BRITISH PHARMACOPŒIA.

SECOND REPRINT, WITH ADDITIONS.

OUR readers will be glad to find that the reprint of the *British Pharmacopœia* with additions is now ready for circulation. It has been long looked for, and will be perused with interest, and we hope with satisfaction. A few necessary corrections have been made in the body of the work, and thirty-four new medicines and substances used in the preparation of medicines have been added in the form of a Supplement. The corrections made in the matter previously published principally relate to specific gravities, and the proportions of ingredients in some of the compound medicines, such as pills, in which undefined quantities of excipients are ordered.

The additions comprise some important medicines of recent introduction, such as hydrate of chloral and nitrite of amyl; some medicines which have become established in use or in favour since the *Pharmacopœia* was published in 1867, such as pepsin, the hypophosphites, and the prepared juices of belladonna and hyoscyamus; and several new forms of medicines, such as chloroform water, mustard paper, liquid extract of liquorice, hypodermic injection of morphia, solution of citrate of magnesia, phosphorated oil, phosphorus pill, compound scammony pill, compound powder of elaterium, com-

pound powder of liquorice; some new forms of suppositories; syrup of chloral; and three new tinctures—namely, tincture of fresh orange-peel, tincture of larch bark, and ammoniated tincture of quinia. Acetic ether, as one of the most agreeable of the compound ethers; nitrate of ammonia, as the source from which nitrous oxide gas is prepared; areca nut, as a valuable anthelmintic; oxide of bismuth, as one of the most definite and reliable of the compounds of bismuth; precipitated oxide or mercury, the form in which mercuric oxide is most easily acted upon by chemical agents, and in which therefore it is used in making the oleates of mercury, are also included among the additions.

We observe in the descriptions given of these medicines that no attempt has been made to describe minutely the processes for the production of definite chemical compounds, such as acetic ether, nitrite of amyl, hydrate of chloral, and hypophosphite of lime; such details being left to the chemical manufacturer, by whom alone these compounds are produced, and all that is required in these cases is that the products shall answer to the descriptions and tests given.

In all cases in which compounded medicines may be supposed to be made by the pharmacist the instructions for making them are given with minuteness, and, when applicable, the tests are also described. Thus we have explicit instructions for making mustard paper, hypodermic injection of morphia, solution of citrate of magnesia, phosphorated oil, pepsin, phosphorus pill, and other preparations of that description.

Several of the medicines now for the first time included in the Pharmacopœia will, we have no doubt, be valuable additions. An authorised formula for hypodermic injection of morphia was much needed. That which is given is for a solution of acetate of morphia containing one grain of the acetate in twelve minims of the injection. Solution of citrate of magnesia has long been used in France as an agreeable and mild purgative. We find here a formula for it as an effervescing draught. Phosphorated oil and phosphorus pill will supply desiderata to those medical men who desire to administer phosphorus in an uncombined state. Pepsin in its concentrated form, as used in medicine, is described, and a process for its production is given, the dose being from 2 to 5 grains. Hitherto we have had no authorised formula for a purgative pill without aloes. We now have a compound scammony pill, consisting of the resins of scammony and jalap, with ginger and soap. The compound powder of elaterium, consisting of elaterium and sugar of milk, will facilitate the safe and convenient administration of a valuable but almost dangerously powerful medicine. Compound powder of liquorice, composed of powdered senna and liquorice with sugar, is derived from the German Pharmacopœia, and is a simplified modification of our confection of senna. The preserved juices of belladonna and hyoscyamus are additions to the similar preparation of hemlock, which has been found the most reliable of the preparations of that drug. The new forms for suppositories, in which soap replaces fat, are intended to obviate an inconvenience that has sometimes been experienced from the melted fat soiling the linen, and thus causing inconvenience to the patient. Tincture of fresh orange-peel may be represented as a contribution to what the Americans have termed elegant pharmacy. Areca nut, given in the form of powder, in doses of two to six drachms, has been highly spoken of as an anthelmintic, and tincture of larch bark has been found a useful remedy in certain bronchial affections.

It will be seen that there is at least a promise of some valuable matter in these additions to our national Pharmacopœia, and if the practical knowledge and judgment displayed in preparing the previously published part of the work have been equally exercised in this, the profession will be indebted to the Medical Council and the Pharmacopœia Committee for their labours in this direction.

Medical News.

ROYAL COLLEGE OF SURGEONS OF ENGLAND. — The following Members were elected Fellows of the College at a meeting of the Council on the 12th inst.:—

Crowther, William Lodewyk, Hobart Town, Tasmania.
(Diploma of Membership dated March, 1841.)
Hodson, Charles F., L.S.A., Bishop Stortford.
(Diploma of Membership dated Dec. 1837.)

At the same meeting the following gentleman was elected an *ad eundem* Fellow of the College:—

Dickson, Thomas, F.R.C.S. Ed., Buxton, Derbyshire.

And the following gentleman was elected an *ad eundem* Member of the College:—

Purves, Wm. Laidlow, L.R.C.S. Ed., Hanover-street, W.

APOTHECARIES' HALL. — The following gentlemen passed their examination in the Science and Practice of Medicine and received certificates to practise on March 12th:—

Clay, James Hemingway, Matlock Bath.
French, Alexander Martin, Chatham.
Klein, Leopold Martial, Lower Norwood.

The following gentlemen passed their Primary Professional Examination on the same day:—

Hope, James William, St. Bartholomew's Hospital.
Murrell, William, University College.

COLLEGE OF PHYSICIANS, IRELAND.—At examinations recently held the following gentlemen obtained the licences in Medicine and Midwifery:—

MEDICINE. — David Brownrigg Bookey, Isaac Bomford Emerson, David Edgar Flinn, Thomas Griffin, Patrick Kennedy, J. Patrick MacSweeny, James Murray, Frederick Robert Murray, Arthur Willoughby Russell, Patrick Joseph Slevin.

MIDWIFERY. — Patrick Kennedy, Joseph Patrick MacSweeny, James Murray, Frederick Robert Murray, Arthur Willoughby Russell.

Dr. Veale, medical officer and public vaccinator, Eastern district, Pately Bridge Union, Yorks, has received a grant from the Local Government Board for successful vaccination.

The Prudential Assurance Company has just issued its twenty-fifth annual report for the year ending the 31st of December, 1873. The total premium income is now £537,711 1s. 11d., showing the great increase of £109,968 10s., and being the largest accession of income during any year of the company's operations.

Medical Appointments.

BANKS, W. M., F.R.C.S.E. (Lecturer on Anatomy at the Liverpool Royal Infirmary School of Medicine), has been appointed Assistant-Surgeon to the Royal Infirmary, vice Harrison, promoted to Surgeon.

BLUNT, T. P., F.C.S., has been appointed a Public Analyst for Montgomeryshire: 21s. per analysis; and 21s. per day, with first-class railway fare, when required to give evidence.

BRUCE, J., M.B., has been appointed Brigade Surgeon 1st Administrative Brigade, Orkney Artillery Volunteers, vice E. Mitchell, L.R.C.S.E., resigned.

BUSHELL, S. W., M.D., has been appointed Teacher of Botany and Zoology at the Royal Medical Benevolent College, Epsom.

EVANS, E., M.R.C.S.E., has been appointed Medical Officer and Public Vaccinator for No. 3 District of the Bridgwater Union, vice Silke, resigned.

EVANS, N., M.D., M.R.C.S.E., has been appointed Medical Officer to the Royal Small Arms Factory, Enfield, vice Beaumont, deceased.

GARLINE, E. W. B., L.R.C.P.Ed., M.R.C.S.E., has been appointed Medical Officer and Public Vaccinator for the Cheshunt District of the Edmonton Union, vice Beaumont, deceased.

JOHNSON, H., M.D., has been appointed a Public Analyst for Montgomeryshire: 21s. per analysis; and 21s. per day, with first-class railway fare, when required to give evidence.

HEAVEN, C. T., M.R.C.S.E., has been appointed Medical Officer and Public Vaccinator for the Colsterworth District of the Grantham Union, vice Priest, deceased.

LEEBODY, J. R., M.A. (Professor of Chemistry, Magee College, Londonderry), has been appointed Public Analyst for the County Londonderry: £20 per annum.

LEONARD, J., M.R.C.S.E., has been appointed Resident Medical Officer to the Royal Infirmary for Children and Women, Waterloo-bridge-road, vice Park, resigned, and appointed a Physician.

LIDBETTER, T. G., M.R.C.S.E., L.S.A., has been appointed Hon. Surgeon to the Rupert-road Branch of the Holloway and North Islington Dispensary, vice Husband, resigned.

MACKENZIE, Dr. S., has been elected Assistant-Physician to the London Hospital.

MACLAREN, R., M.D., L.R.C.S.Ed., has been appointed a Surgeon to the Cumberland Infirmary, Carlisle, vice Page, resigned.

MOINET, F. W., M.D., F.R.C.P.Ed., has been appointed Lecturer on Materia Medica at Surgeons' Hall, Edinburgh, vice Fraser, appointed Medical Officer of Health for the Altrincham Rural Sanitary District, combined with others.

MOORE, W., M.R.C.S.E., has been appointed Medical Officer and Public Vaccinator for the Hartlebury District of the Droitwich Union, vice Lamb, deceased.

READ, C., M.R.C.S.E., has been appointed Medical Officer for District No. 3 of the City of London Union.

RICHMOND, O. E., L.R.C.P.L., M.R.C.S.E., has been appointed Resident Surgeon to the Royal Sea-Bathing Infirmary, Margate, vice Dixon, resigned.

SAUNDERSON, R., jun., M.D., L.R.C.S.I., has been appointed Medical Officer, Public Vaccinator, and Registrar of Births &c., for the Rhode Dispensary District of the Edenderry Union, King's County, vice Atkin, deceased.

SEQUEIRA, H. L., M.R.C.S.E., has been appointed Medical Officer for District No. 5 of the City of London Union.

SMART, A., M.D., F.R.C.P.Ed., has been appointed Examiner in Chemistry at the Royal College of Physicians, Edinburgh, vice Fraser, appointed Medical Officer of Health for the Altrincham Rural Sanitary District, combined with others.

SMITH, J., M.D., F.R.C.S.Ed., has been appointed a Consulting Surgeon to the Edinburgh Dental Dispensary.

STOWERS, J. H., M.R.C.S.E., has been appointed Administrator of Chloroform &c. to St. Bartholomew's Hospital, vice A. Cumberbatch, F.R.C.S., resigned.

TALBOT, J. B., M.R.C.S.E., has been appointed Assistant Medical Officer to the Salop and Montgomeryshire Counties Lunatic Asylum, vice Percival.

TIVY, W. J., L.R.C.P.Ed., L.R.C.S.Ed., has been appointed Medical Officer for the Dilwyn District of the Weobley Union, vice Beavan, resigned.

TOWNSEND, R. N., L.K.Q.C.P.I., L.R.C.S.I., has been appointed Medical Officer and Public Vaccinator for the Queenstown Dispensary District of the Cork Union, vice Lawlor, resigned.

WALLACE, Dr. S. L., has been appointed Parochial Medical Officer for Inverkip, Renfrewshire.

WESTON, W. B., M.R.C.S.E., has been appointed Medical Officer and Public Vaccinator for the Rocester District of the Uttoxeter Union, vice Cooke, resigned.

Births, Marriages, and Deaths.

BIRTHS.

ATKINSON.—On the 17th inst., at Ashford, Kent, the wife of Charles S. A. Atkinson, M.R.C.S., of a son.

CHEADLE.—On the 12th inst., at Hyde-park-place, Cumberland-gate, the wife of W. B. Cheadle, M.D., of a son.

DUDGEON.—On the 7th inst., at Montagu-square, the wife of R. E. Dudgeon, M.D., of a son.

HUTSON.—On the 16th of Jan., at St. Michael's Lodge, Bridgetown, Barbadoes, the wife of Charles Hutson, M.B., M.R.C.S.E., of a son.

MACFARLANE.—On the 13th inst., at Thornhall House, Polmont, N.B., the wife of Alex. W. Macfarlane, M.D., of a daughter.

MURDOCH.—On the 4th inst., the wife of Donald Murdoch, M.R.C.S., L.S.A., of Union-road, Rotherhithe, of a son.

ROBINSON.—On the 12th inst., at Dorchester, the wife of J. D. Robinson, L.R.C.P., M.R.C.S., L.S.A., of a son.

STEWARD.—On the 13th inst., at St. John's-square, Wolverhampton, the wife of T. T. Steward, M.R.C.S.E., L.S.A., of a daughter.

MARRIAGES.

FOX—REDDIE.—On the 10th inst., at Holy Trinity Church, Chelsea, Charles Henry Fox, M.D., of Brislington, near Bristol, to Sarah Charlotte, daughter of Major-General Geo. B. Reddie.

MUMMERY—LOCKHART.—On the 12th inst., at Lewisham, John Howard Mummery, M.R.C.S.E., to Mary Lily, daughter of William Lockhart, F.R.C.S.E.

PENNINGTON—BRAITHWAITE.—On the 17th inst., at St. Silas Church, Liverpool, Thomas Pennington, M.R.C.S., L.S.A., to Jane, eldest daughter of the late Edward Braithwaite, Esq., of Ulverston.

DEATHS.

EBBAGE.—On the 15th inst., Thos. Ebbage, F.R.C.S.E., of York-terrace, Leamington, aged about 60.

GOODWIN.—On the 28th ult., John Wycliffe Goodwin, M.D., of Bournemouth, formerly of Bury St. Edmunds, aged 48.

HUTSON.—On the 22nd of Jan., at St. Michael's Lodge, Bridgetown, Barbadoes, Caroline, wife of Charles Hutson, M.B., M.R.C.S.E., aged 26.

KEIRAN.—At Belturbet, Co. Cavan, James Keiran, M.D.

REID.—On the 4th of Jan., at Geelong, Australia, Mary Jane Cumberland, eldest daughter of the late Geo. Synnot, and wife of David Boswell Reid.

ROCHE.—On the 4th inst., Wm. S. Roche, M.B., Surgeon R.N. (retired), of Rectory-place, Woolwich, aged 44.

WILLIAMS.—On the 8th inst., at Walworth-road, Kennington, John James Williams, M.R.C.S.E., L.M., L.S.A., District Medical Officer and Public Vaccinator, late Army Medical Staff attached to Ottoman Army, aged 57. Friends will please accept this intimation.

WOODMAN.—On the 15th inst., Frederick Woodman, M.D., of Deal, aged 34.

BOOKS ETC. RECEIVED.

Dr. Allen on Aural Catarrh and Curable Deafness.
Mr. Maclaren: Training in Theory and Practice.
Obstetrical Transactions. Vol. XV.
Dr. Gardner: Longevity.
Sir H. Thompson: Leçons Clinique sur les Maladies des Voies Urinaires.
The Pickwick Club. By Charles Dickens.
Mr. Hyslop: Cheerful Words.
Dr. Fritze: Schwalbach.
Nouveau Dictionnaire de Médecine et de Chirurgie.
The Obstetrical Journal. Vol. I.
Food, Air, and Water. Chemical News.

METEOROLOGICAL READINGS

(Taken by Steward's Instruments).

THE LANCET OFFICE, MARCH 19TH, 1874.

Date.	Barometer reduced to Sea Level, and 32° F.	Direction of Wind.	Wet Bulb.	Dry Bulb	Solar Radia. in Vacuo	Max. Temp. Shade	Min. Temp.	Rainfall.	Remarks at 8 A.M.
Mar. 13	30·36	N.W.	31	33	...	47	29	0·27	Fine
„ 14	30·51	W.	42	46	...	52	36	...	Fine
„ 16	30·35	W.	45	48	...	55	43	0·05	Fine
„ 17	30·26	W.	47	49	...	56	41	...	Raining
„ 18	30·09	W S W	48	49	...	58	48	0·01	Raining
„ 19	30·14	W N W	40	43	...	...	38	0·05	Fine

Notes, Short Comments, and Answers to Correspondents.

IN consequence of the great amount of space taken up by our reports of the discussions on Pyæmia, Cancer, &c., we are compelled to postpone the publication of several important articles and communications.

FRENCH MEDICAL PUBLISHERS.

THE house of Baillière is known wherever French medical books are read, and many will hear with satisfaction that the Legion of Honour has just been awarded to M. Emile Baillière, son of the founder. The house has been in existence half a century, and works which do infinite credit to authors and publishers have issued from the firm. We would merely mention Littré's Hippocrates and the Dictionary of Medicine, now going through the press. The talented editor of *L'Union Médicale*, M. Amédée Latour, mentions, with his usual good-nature, many traits of the Messrs. Baillière, proving that they are fully alive to the interests of authors—a feature of character not every day noticed.

Enquirer.—In the absence of a contract there is no compulsion binding the principal; but he ought to pay in accordance with custom. Our correspondent must be content to get a lesson out of his experience to contract beforehand for payment of expenses.

Mr. W. H. Parr.—Messrs. Knight and Company, 90, Fleet-street.

WHY IS THE NAVAL MEDICAL SERVICE SO UNPOPULAR?

To the Editor of THE LANCET.

SIR,—This is a question, considering all that has been recently said on the subject in THE LANCET and elsewhere, which may be very fairly asked. Why is there such a great desire on the part of the junior officers to leave the service? There must be something radically wrong when we find that many of those who come into the service with a firm intention of spending their lives in it, and overcoming any little inconvenience necessarily attendant on a sea life, are so anxious to leave, or, remaining, are discontented. Let me mention a few of the reasons which occur to me.

1st. In no service is relative rank of more importance than in the Royal Navy. The surgeon (I use the new name) enters with the rank of sub-lieutenant only; yet he is a ward-room officer in other respects. All other civilian professional officers enter in the higher grades; a naval instructor, who can scarcely be considered a professional man, ranking on entry with a lieutenant. On this question of rank, which to outsiders may seem a very paltry one indeed, often hinges the whole happiness of the young surgeon's life; through his inferior rank he is often exposed to snubbing and covert insults, even from those very officers who dine at the same table with him. One of these, somewhat his senior, may *order* him to give place to him in a boat, even when off duty and in plain clothes, and that, too, before the seamen and others in the boat; yet this is only one of the many instances in which he may have a slight put upon him. He is very often given a cabin far inferior in every way to a warrant officer's, and in a part of the ship not thought good enough for any other ward-room officer. The "Instructions" entitle him to a cabin. "Medical officers are to have cabins." They are acted up to in the letter, not in the spirit. He also sees a paymaster, though junior to, getting a cabin much superior to a senior staff surgeon 1st class, and that, too when the matter has been duly represented. Such things strike the surgeon on entry forcibly and at once.

2ndly. As regards pay. On entry the pay is sufficient for his wants; but the future has to be looked to, and it is then found that the pay does not increase in equal proportion with that of other ranks; as a paymaster, for instance, gets far more pay than a staff surgeon 1st class.

3rdly. As regards promotion. Anyone glancing at the Navy List can satisfy himself in a moment as to how slow that is. A surgeon cannot hope to get a step sooner than thirteen years. How long must he wait for another!

These, Sir, are facts. They are only a few amongst the many; but it has been well said that one fact is worth a thousand arguments. Much has been done recently by the authorities to benefit a class of officers educated in the service. Will nothing be done to encourage the naval surgeon, to induce good men to enter the service, or is the health of the navy a matter of secondary importance? Your obedient servant,

February 15th, 1874. NAVAL SURGEON.

The Present Writer omits to notice the different lengths of service of the two individuals whose widows have been pensioned. If the solicitor's widow had been paid at the same rate for length of service as the doctor's widow, the pension to the former would have been much greater.

Foves Centralis.—The matter shall receive attention.

Inquirer, (Birmingham.)—The discovery of spectrum analysis, for the present perfection of which we owe so much to Kirchhof, would seem to have been largely foreshadowed by Dr. W. H. Wollaston, who in 1803 observed a few dark lines in the solar spectrum. Dr. Wollaston also, we believe, was the first to introduce to notice the metals palladium and rhodium.

Fractured Femur.—The fee of five guineas is, we think, fair and proper under the circumstances. The view taken by our correspondent respecting the Poor-law scale is, in our opinion, a just one.

Mr. F. T. Young, (Bristol.)—The fee for the class of Medico-Psychology in Edinburgh University under Professor Laycock is now £3 3*s*.

Dr. Maurice G. Evans.—Illegal, we believe.

Mr. Bultcel.—Many thanks. The subject is noticed this week.

PYÆMIA IN PRIVATE PRACTICE.

To the Editor of THE LANCET.

SIR,—As you are anxious to have notes on pyæmia in private practice, I send you the only case that I have any record of.

In the early part of last year I attended Mrs. J. B—— in her first confinement. She was a tall, strong, healthy-looking woman of twenty-five. Labour began about ten in the morning of Friday, and went on slowly all day. I was sent for about ten in the evening, the pains being very severe. The child's head descended very slowly, and at eleven on Saturday morning I delivered her with the forceps (Zeigler's), without difficulty, of a healthy female child. All went well for a week; but early in the morning of the seventh day she had a rigor, followed by pain and swelling in the whole right leg. An abscess formed in the foot, which I opened, and gave exit to about a teacupful of healthy pus. She improved after this for a few days, although the swelling still continued in the thigh, which was hard and hot. I suspected the presence of deep-seated matter; but owing to the thickness of adipose tissue it was difficult to diagnose. The femoral vein was hard and cord-like; the pain very severe. I saw Mr. E. B. Hicks, of Easingwold, in consultation, who agreed with me in thinking that deep-seated matter was present. I made an incision of three inches through the deep fascia in the outer side of the middle third of the thigh, which gave exit to a considerable quantity of ichorous pus. We then bandaged the thigh, dressing the wound with tenax. The patient still had rigors and profuse sweats, for some days hovering between life and death; temperature 105°. She took a large quantity of brandy, porter, and bark, fortunately never losing the power of taking nourishment. Matter again formed in the calf of the leg, which was absorbed. I am glad to be able to add that she eventually recovered completely.

I am able to assign no reason for the occurrence of these symptoms. I was at the time attending no case of the sort. But on referring to my notes I find that at the time seven or eight patients were suffering from abscesses in the hand. In fact, from the last week in December, 1872, till April or May, 1873, three or four patients were under treatment without intermission. I am very careful as to conveying infection, always wearing a different suit of clothes for night work, and it so happened that the night I attended Mrs. B—— I had to be fetched from a dinner party, and was wearing a dress suit.

I am sorry to say that although this case ended well, and I was more than ordinarily careful, I must in justice add another case.

About a week after Mrs. B——'s name was erased from my visiting-list I attended a young woman, a domestic servant, in her first confinement. She was aged eighteen, very robust and healthy. As she had been seduced she was at home in a cottage, very much overcrowded. She had a very good labour, and, against my orders, was up and sitting at the open door on the third day. She also washed in, and drank large quantities of, cold water. On the sixth day she had rigors, followed by profuse sweats; temperature 103° to 105° (the certificated thermometer). She had brandy, milk, and brandy-and-egg mixture. Acute puerperal peritonitis, with effusion, came on, and she died in ten days. Whether it was an idiopathic case or communicated by me I do not know; but I fear it was the latter.

I may remark that my forceps were scrupulously clean, and that the lochia was discharged until the sixth day in Mrs. B——'s case.

I am, Sir, your obedient servant,

Stillington, Yorks, Feb. 23rd, 1874. F. SIDNEY GRAMSHAW.

To the Editor of THE LANCET.

SIR,—The following is the only case of pyæmia which I have attended during a practice of more than thirty years, including eleven of parochial practice. It appears to me interesting on account of the unusual length of the incubative period, the steady rise of the temperature, the great rapidity of the respiration during the last day or two, as well as the close resemblance of the symptoms to those of typhoid fever. There was no post-mortem examination; but no doubt the death was by embolism. The house is large and well ventilated, situated in a wide street, nor could I discover any defect in its hygienic relations.

Dec. 12th.—10 P.M.: Mr. P——, aged twenty-four, a month back had an abscess in left forefinger, caused by a splinter having been thrust into it. He pouliticed it, but had no professional attendance. Two weeks back he noticed some stiffness in the arm in the region of the biceps; in another week this part of the arm became swollen. Now there is what appears like phlegmonous erysipelas of left arm, which is very tender on pressure. Pulse 120; temperature 102·4°. Diarrhœa three days. Very frequent motions this morning, like pea-soup.

13th.—10 P.M.: Diarrhœa. Pulse 100; temperature 102·8°; occasional nocturnal incoherency.

14th.—10 P.M.: Temperature 103·8°; pulse 100; epistaxis; refuses food.

15th.—10 P.M.: Very profuse perspiration; temperature 103·4°; pulse 112, faltering; respiration panting, 60 per minute, but no râles; incoherency increased; attempts to get out of bed.

16th.—10 P.M.: Very copious perspiration; temperature 103·6°; pulse 120; respiration 40; diarrhœa, pea-soup evacuations; increasing incoherency; has to be held in bed, but still answers questions rationally; swelling of arm increasing, pits on pressure.

17th.—10 A.M.: Temperature 104°; pulse 120; respiration 65; arm trembles when extended; diarrhœa; urine smoky and albuminous, sp. gr. 1015·5, fragrant (sweet briar) smell; constant muttering delirium about business affairs; swollen arm livid; tumefaction extended to shoulder and forearm. 10 P.M.: Temperature 104·4°; pulse 120; respiration 72; sibilous râles profuse sweating.

18th.—10 A.M.: Temperature 105°; pulse 120; respiration 72; carphology, which the nurse assures me only commenced last night; no sleep, and required two persons to hold him in bed; brachial swelling has become hard, and no longer pits.—10 P.M.: Temperature 105·6°; pulse 100, fluttering; respiration 78; muttering delirium; diarrhœa; urine, sp. gr. 1017.

19th.—Died at 2 A.M. whilst apparently sleeping calmly.

The following is a case of phlebitis after delivery, which appears to me somewhat unique on account of the paucity of the symptoms prior to disease making its appearance in the joints.

Mrs. W——, aged forty. Delivery at full term of pregnancy. Placenta adherent by its whole maternal aspect to the posterior surface of uterus; the adhesions extremely firm, and the separation effected very slowly and with great difficulty. The uterus contracted well; there was no flooding, and no abdominal tenderness nor other sign of uterine or peritoneal inflammation. The only symptom during the first fortnight to cause the least anxiety was the steady persistence of the pulse at 120. At the end of the second week the patient began to complain of pain in the left shoulder-joint, which in a fortnight afterwards suppurated, and discharged a large quantity of bloody pus. There was no hardness or tenderness about the superficial veins or absorbents, but the arm became œdematous. The elbow-joint subsequently suppurated. It was a considerable time before the affected joints regained their mobility.

I am, Sir, your obedient servant,

Blackheath, March 7th, 1874. J. HARMAR SMITH.

Cosmos.—Yes; there are people who still believe in the survival of Livingstone. At a recent sitting of the Berlin Geographical Society, Prof. Koner delivered an address, in which he refused to accept Livingstone's death as a fact, and hoped that it might turn out to be a false alarm.

Mr. J. W. Richards.—Consult a surgeon.

One who has Passed.—So long as the professional examinations are simply "pass," and the successful candidates are not classified in any way, it would be useless to pursue any other plan with a mere preliminary examination.

THE ELASTIC LIGATURE.

To the Editor of THE LANCET.

SIR,—After reading in THE LANCET of March 7th the abstract of Dr. Quinlan's lecture on the use of the Elastic Ligature for the removal of a growth from the Vulva, I am induced to forward the following particulars of a rather analogous case.

My patient, a lady whom I had previously attended in two confinements, consulted me in January last for an attack of diarrhœa, which soon disappeared, apparently leaving her in an emaciated and generally miserable condition, for which I could not account. On expressing my surprise that she should from so slight an illness remain in such a low condition, she gave me the following history of her trouble:—She had been married four years, and suffered excessive pain during coitus. When about three months pregnant she consulted a distinguished specialist, who comforted her by the information that after the birth of her child the pain would be removed. As this was not the case, she again and again consulted him, and various remedies were used without success. She again became pregnant, and was confined about eighteen months ago. The pain had now become so intolerable that all intercourse had ceased for some time, and she begged of me to examine her and see if something might not be done to relieve her. I feared from her appearance that there existed some malignant growth either of the rectum or vagina; but upon examination I found growing from the posterior fourchette, more upon the left side than the right, what appeared to be an exaggerated third nympha, in breadth about an inch, but much broader at its free extremity, in length an inch and a half; it was most exquisitely sensitive, and my patient informed me that even walking produced great pain. The rectum was perfectly healthy, and free from hæmorrhoids.

I advised immediate removal, and, as she strongly objected to the knife, I determined to use the elastic ligature. On Feb. 11th I therefore passed through the base of the growth a double ligature, and tied them on either side. In three days the whole of it had separated, leaving two small granulating spots, which were completely healed in three or four days. She is now quite well and free from pain.

What struck me as remarkable in this case was the fact, that after the application of the ligature there was literally no pain whatever, and she did not know when the growth had separated; also that the cause of all her troubles had not been discovered before—a result that may be attributed to the method of examination. In the first instance, she would be examined in the consulting-room in the obstetric position, and when the fingers were introduced into the vagina the growth would be pushed within, and neither seen nor felt. It was evidently treated as a case of vaginismus. I placed the patient upon her back, and upon separating the labia the offender was readily discovered.

I remain, Sir, yours truly,

Small Heath, March 10th, 1874. HENRY KETTLE, M.R.C.S. Eng.

Alumnus, (York.)—The examiners in Medicine at the University of Edinburgh for the current year are Dr. William Robertson, Dr. Benjamin Bell, and Dr. William Dumbreck—the examiners for last year.

Mr. J. Waring-Curran is thanked.

DERANGED AUDITORY FUNCTION.

To the Editor of THE LANCET.

SIR,—In reply to "Malleus," I can say that several cases of tinnitus which have come under my observation during the last twelve months have been due to an obstructed Eustachian tube, with a depressed or over-concave tympanic membrane, and have been greatly relieved by occasional inflation of the tympanum with air, after Politzer's method. Not everyone possesses a Politzer's bag; but in the bag, or rather bags, of the ether spray I find a substitute often sufficiently effective. The *modus operandi* is described in the most recent editions of our surgical compendiums. If requested, however, I should be glad to give further information on the subject.

Yours obediently,

G. B. FERGUSON, M.B., M.A.,

Cheltenham, March 16th, 1874. Surgeon to the Cheltenham Hospital.

Curiosa (Stonehaven) asks if anyone has ever seen or heard of an ordinary-sized hen's egg entirely filled with albumen, and without any coloured yelk. He has had one of the kind to breakfast. A correspondent in the *Scotsman* says, however, his hens have laid several such this season; but none of them were larger than a pigeon's egg.

Mr. William Hyslop (Stretton House, Salop.)—The volume has been received, and shall be noticed in an early number.

Mr. Fred. Laslett is thanked.

UNNECESSARY INQUESTS.

To the Editor of THE LANCET.

SIR,—I have only just seen the *Welshman* newspaper of the 13th instant, which contains an extract from your impression of the 7th instant, in which notice is taken of my refusal to give a certificate of the cause of death of Ann Jones, and the consequent inquest. There appears to be some misapprehension or want of *correct* information on the part of the *Welshman*, which I trust you will permit me to rectify. The simple facts are these. On February 13th I was called in to attend Ann Jones, and continued my attendance daily up to the morning of the 16th. There were then no alarming symptoms. From some cause or other her friends very shortly after my last visit, as you will see on referring to the husband's evidence at the inquest, thought proper to consult Dr. Hopkins, who prescribed some pills for her without seeing her, and not on my behalf. During the afternoon, shortly after she had taken one of the pills, a messenger was sent for me, requesting me to see her forthwith. I was absent from home at the time; but before the messenger returned she was dead. Of course I was ignorant of what the pills contained until the prescription was shown to me at the inquest.

The question as it affects me is not whether an inquest was necessary or otherwise, but whether I was justified in giving a certificate of death when the patient had been taken from my hands by another qualified medical man. I say, certainly not. Your obedient servant,

Carmarthen, March 17th, 1874. E. P. DAVIES.

Mr. Francis Cufaude.—By asking for copies of the Rules from the Secretary of one or more of the Provident Dispensaries, as of Camberwell or Northampton.

Beta.—We can only suggest to our correspondent to address the Secretary of the Epileptic Hospital.

Dr. McKeown's letter shall be kept in mind.

Mr. William Rayner.—We should suggest half a pint less beer for dinner, and half a pint of milk on going to bed.

COMMUNICATIONS, LETTERS, &c., have been received from—Dr. R. Barnes, London; Dr. Anstie, London; Dr. Burdon-Sanderson, London; Dr. Gowers, London; Mr. Hutchins; Dr. Rumsey, Cheltenham; Mr. Jeaffreson, Newcastle; Mr. Hyslop, Church Stretton; Dr. Maudsley, London; Mr. Kershaw, London; Mr. Numa Rât, London; Dr. Douglas, Hounslow; Mr. Hastings, Boston, U.S.; Mr. Laslett, H.M.S. *Pallas*; Surgeon-Major Longhurst, Portland; Dr. Stewart, Barnsley; Dr. Hawkes, Alton; Dr. Redwood, London; Mr. Jackman, Barnstaple; Mr. Richards, Carlisle; Dr. Dowse, London; Dr. MacCormac; Mr. J. Waring-Curran; Mr. Gregg, London; Mr. Murdoch, London; Dr. Battye, London; Dr. Luke Peacan, Croydon; Dr. Wood, London; Mr. Griffiths, Hanley; Mr. Wainwright, Barnsley; Mr. Simpson, Devizes; Mr. Harper, London; Mr. J. Leonard, London; Dr. Hodgson, Preston; Dr. Fraen, South Shields; Mr. Pearce, Ipswich; Mr. Perrin, Temple Cloud; Dr. Holdsworth, Wakefield; Mr. Simpson, West Rainton; Dr. Tate, Woodbridge; Dr. Fothergill, Durham; Dr. Will, Aberdeen; Mr. Allen, Hastings; Dr. Yeo, Dublin; Mr. Myers, Liverpool; Dr. Silvester, Tunbridge Wells; Mr. Parkinson, Milburn; Mr. Franklin, Newcastle; Mrs. Toyne, Ancoats; Mr. Kennedy, Dublin; Dr. Gill, Dover; Mr. Williams, Bryn'r Aber; Dr. Drewry, Walsall; Mr. Richards, Glasgow; Dr. Archer, Barbadoes; Dr. Hall, Swadlincote; Mr. Kilburn, Auckland; Mr. Everett, Nottingham; Mr. Baines, Corsham; Mr. Townley, Liverpool; Mr. Youldon, London; Mr. Shield, London; Dr. Addison, London; Dr. Mason, Sudbury; Mr. Bernard, Portland; Mr. Dorman, St. Helens; Mr. Liddell, Alfreton; Mr. Walker, Hanley; Mr. Hennessy, Bradford; Dr. L. Propert, London; Mr. Marrack, Truro; Mr. Holroyd, Runcorn; Miss Deakins, London; Mr. Kavanagh, Ware; Dr. A. Thompson, London; Dr. Diver, Southsea; Mr. Davies, Carmarthen; Mr. Bartleet, Edgbaston; Mr. Jukes, Halifax; Dr. Veale, Ripley; Dr. Galton, Atcheen, Sumatra; Dr. Thursfield, Bridgnorth; Dr. Tibbits, Bristol; Dr. Veitch, Middlesborough; Mr. Estcourt, Manchester; Mr. Horatio Neill; Mr. Downes, Croydon; Dr. Maurice Evans; Mr. Rorie; Dr. Arlidge; Dr. Bullmun, Falmouth; Mr. Cufaude; Dr. Macfarlane, Polmont; Mr. Terry, Bradford; Mr. Buckton, Wrexham; Dr. Wallich, Herne Bay; Mr. Harben, London; Dr. Rayner, Solihull; Mr. Banks, Ramsgate; Mr. Macfarlane, Glasgow; Mr. Blasson, Edgware; Mr. Turner, H.M.S. *Indus*; Mr. Williams, Bristol; Mr. Gordon Brown; Mr. Delamotte, Swanage; Mr. Lloyd, Woolwich; Mr. M'Keown, Belfast; Mr. Gilmour; Mr. Hyland, Tonbridge; Mr. Drew, London; Mr. Hopkins, Smethwick; Mr. Peacock, Harewood; Dr. Jeffries, Aylesford; Dr. King, Widnes; Mr. Messenger, Glasgow; Mr. Benhall, Halstead; Mr. Cowell, London; Mr. Whincup, London; Rev. E. G. Banks, Margate; Mr. Shirley, Holbeck; Dr. Bruce, Kirkwall; Dr. Jay, Chippenham; Dr. Anderson, Glasgow; Mr. Parry, Bridgnorth; Mr. Stephenson, Hornby; The Director-General of the Medical Department of the Navy; The Quekett Microscopical Club; S. T. F.; The Authoress; Enquirer; Cedric; J. M. M.; Povea Centralis; J. D.; &c. &c.

Macclesfield Chronicle, Drawing-room Gazette, Newcastle Daily Chronicle, Carlisle Patriot, Manchester Guardian, Hospital Gazette, Surrey Advertiser, Cork Constitution, East London Observer, Scotsman, Whitby Gazette, Bury Times, Glasgow News, Manchester Examiner, Liverpool Daily Post, Druggist and *St Pancras Gazette* have been received.

Medical Diary of the Week.

Monday, March 23.

ROYAL LONDON OPHTHALMIC HOSPITAL, MOORFIELDS.—Operations, 10½ A.M.
ROYAL WESTMINSTER OPHTHALMIC HOSPITAL.—Operations, 1½ P.M.
GUY'S HOSPITAL.—Operations on the Eye, 1½ P.M.
ST. MARK'S HOSPITAL.—Operations, 9 A.M. and 2 P.M.
METROPOLITAN FREE HOSPITAL.—Operations, 2 P.M.
ROYAL COLLEGE OF SURGEONS OF ENGLAND.—4 P.M. Mr. W. K. Parker, "On the Structure and Development of the Skull in the Vertebrata."
MEDICAL SOCIETY OF LONDON.—8 P.M. Mr. Thomas Bryant: "Foreign Body discharged through Abdominal Parietes."—Dr. Cockle and Mr. J. Blackstone, "On a Case of Labial Carbuncle followed by Pyæmia."—Dr. Tuchman, "On a New Instrument to aid in Diagnosing Disease of the Bladder and Kidneys."

Tuesday, March 24.

ROYAL LONDON OPHTHALMIC HOSPITAL, MOORFIELDS.—Operations, 10½ A.M.
ROYAL WESTMINSTER OPHTHALMIC HOSPITAL.—Operations, 1½ P.M.
GUY'S HOSPITAL.—Operations, 1½ P.M.
WESTMINSTER HOSPITAL.—Operations, 2 P.M.
NATIONAL ORTHOPÆDIC HOSPITAL.—Operations, 2 P.M.
WEST LONDON HOSPITAL.—Operations, 2 P.M.
ROYAL INSTITUTION.—3 P.M. Prof. Tyndall, "On the Physical Properties of Liquids and Gases."
ROYAL MEDICAL AND CHIRURGICAL SOCIETY.—8½ P.M. Dr. J. M. Cuningham (Sanitary Commissioner with the Government of India): "Recent Experience of Cholera in India."—Mr. R. Brudenell Carter, "On an Improved Method of Abscission of the Anterior portion of the Eyeball."

Wednesday, March 25.

ROYAL LONDON OPHTHALMIC HOSPITAL, MOORFIELDS.—Operations, 10½ A.M.
MIDDLESEX HOSPITAL.—Operations, 1 P.M.
ST. MARY'S HOSPITAL.—Operations, 1½ P.M.
ROYAL WESTMINSTER OPHTHALMIC HOSPITAL.—Operations, 1½ P.M.
ST. BARTHOLOMEW'S HOSPITAL.—Operations, 1½ P.M.
ST. THOMAS'S HOSPITAL.—Operations, 1½ P.M.
KING'S COLLEGE HOSPITAL.—Operations, 2 P.M.
GREAT NORTHERN HOSPITAL.—Operations, 2 P.M.
UNIVERSITY COLLEGE HOSPITAL.—Operations, 2 P.M.
LONDON HOSPITAL.—Operations, 2 P.M.
SAMARITAN FREE HOSPITAL FOR WOMEN AND CHILDREN.—Operations, 2½ P.M.
CANCER HOSPITAL.—Operations, 3 P.M.
ROYAL COLLEGE OF SURGEONS OF ENGLAND.—4 P.M. Mr. W. K. Parker, "On the Structure and Development of the Skull in the Vertebrata."
ROYAL COLLEGE OF PHYSICIANS OF LONDON.—5 P.M. Dr. Sibson, "On the Influence of Bright's Disease in the Production of Inflammation."

Thursday, March 26.

ROYAL LONDON OPHTHALMIC HOSPITAL, MOORFIELDS.—Operations, 10½ A.M.
ST. GEORGE'S HOSPITAL.—Operations 1 P.M.
ROYAL WESTMINSTER OPHTHALMIC HOSPITAL.—Operations, 1½ P.M.
UNIVERSITY COLLEGE HOSPITAL.—Operations, 2 P.M.
ROYAL ORTHOPÆDIC HOSPITAL.—Operations, 2 P.M.
CENTRAL LONDON OPHTHALMIC HOSPITAL.—Operations, 2 P.M.
ROYAL INSTITUTION.—3 P.M. Prof. W. C. Williamson, "On Cryptogamic Vegetation: Ferns and Mosses."
HUNTERIAN SOCIETY.—8 P.M. Open Meeting.

Friday, March 27.

ROYAL LONDON OPHTHALMIC HOSPITAL, MOORFIELDS.—Operations, 10½ A.M.
ST. GEORGE'S HOSPITAL.—Ophthalmic Operations, 1½ P.M.
ROYAL WESTMINSTER OPHTHALMIC HOSPITAL.—Operations, 1½ P.M.
GUY'S HOSPITAL.—Operations, 1½ P.M.
ROYAL SOUTH LONDON OPHTHALMIC HOSPITAL.—Operations, 2 P.M.
CENTRAL LONDON OPHTHALMIC HOSPITAL.—Operations, 2 P.M.
ROYAL COLLEGE OF SURGEONS OF ENGLAND.—4 P.M. Mr. W. K. Parker, "On the Structure and Development of the Skull in the Vertebrata."
ROYAL COLLEGE OF PHYSICIANS OF LONDON.—5 P.M. Dr. Sibson, "On the Influence of Bright's Disease in the Production of Inflammation."
QUEKETT MICROSCOPICAL CLUB.—8 P.M. Mr. E. T. Newton, "On the Preparation of Microscopic Sections of Soft Tissues."
ROYAL INSTITUTION.—8 P.M. Weekly Evening Meeting.—9 P.M. Prof. A. C. Ramsay, "On the Physical History of the Rhine."
CLINICAL SOCIETY OF LONDON.—8½ P.M. Dr. Cayley, "On a Case of Hæmoptysis."—Mr. W. Haward, "On a Case of Blood-cyst of Hand."—Dr. Packard, of Philadelphia: "Bracketed Splint for Excision of the Knee or for Compound Fracture" (communicated by Mr. Callender).

Saturday, March 28.

HOSPITAL FOR WOMEN Soho-square.—Operations, 9½ A.M.
ROYAL LONDON OPHTHALMIC HOSPITAL, MOORFIELDS.—Operations, 10½ A.M.
ROYAL WESTMINSTER OPHTHALMIC HOSPITAL.—Operations, 1½ P.M.
ST. BARTHOLOMEW'S HOSPITAL.—Operations, 1½ P.M.
KING'S COLLEGE HOSPITAL.—Operations, 1½ P.M.
ROYAL FREE HOSPITAL.—Operations, 9 A.M. and 2 P.M.
CHARING-CROSS HOSPITAL.—Operations, 2 P.M.
ROYAL INSTITUTION.—3 P.M. Mr. C. T. Newton, "On Wood's Discoveries at Ephesus."

TERMS FOR ADVERTISING IN THE LANCET.

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For every additional line	0	0	6	For a page	5	0	0

The average number of words in each line is eleven.

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Agent for the Advertising Department in France—

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